THREE ESSAYS ON LOCAL AND FEDERAL PROGRAMS SERVING CHILDREN WITH DISABILITIES

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SERVING CHILDREN WITH DISABILITIES

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ABSTRACT

In 2017, 6.7 million children received special education services and 1.2 million children received Supplemental Security Income. Despite the reach of these two programs, little research has examined how local, state, and federal policies interact with these two program. This dissertation is comprised of three essays examining local and federal policies affecting children with disabilities. In Chapter 1, I use administrative student level records from the state of North Carolina and regression discontinuity methods, to corroborate earlier research suggesting that the youngest children in the classroom are more likely to receive special education services relative to their older peers. Children born the month before the school cutoff date are 1.75 percentage points (16%) more likely to receive special education in grade 3 relative to their peers born the month after the cutoff date. Importantly, I find that the gap in special education placement does not diminish with school tenure. In grade 12, children born the month before the cutoff date are still 3.84 percentage points (42%) more likely to receive special education services relative to their peers born the month after the cutoff date. Thus, I find evidence of a negative feedback loop in which the youngest children are placed on a lower track at the onset of their schooling, from which they generally do not recover.

In Chapter 2, I document a direct pathway from receipt of special education to SSI using a two-sample fuzzy regression discontinuity design. First, using administrative records from North Carolina, I corroborate earlier findings that children born the month before the kindergarten entry eligibility cutoff date are more likely to receive special education services relative to children born the month after the school cutoff date. Next, using National Health Interview Survey respondents linked to Social Security Administration records, I document that the chil-

dren born just before the cutoff date are 0.78 percentage points (or 30%) more likely to apply for and 0.55 percentage points (or 59%) more likely to receive an award for SSI between the ages of 5 and 12 relative to children born just after the school cutoff date. I find no increase in awards among groups unlikely to be affected by the relationship between school starting age and special education; these include children with physical impairments or those too young for school enrollment. Two-sample fuzzy RD estimates indicate that a 1 percentage point increase in the fraction of children receiving special education services induces a 0.16 percentage point (or 10%) increase in the fraction of children with an SSI award. Back of the envelope calculations suggest that approximately 18% of the growth in the SSI caseload can be attributed to rising rates of special education and spillovers between these two programs.

In Chapter 3, I test how exposure to the Earned Income Tax Credit (EITC) affects the likelihood a child receives SSI payments between the ages of 15 and 18. Exogenous variation in exposure to the EITC is derived from the maximum credit available to the child in his state of residence each year between the ages of 0 and 18. Reduced-form estimates indicated that exposure to an additional $1,000 each year reduces the likelihood that a child receives income from SSI by 0.34 percentage points (26%). Exposure to a larger EITC between the ages of 13 and 18 has the largest impact on SSI receipt. I find no evidence that the primary channel through which the EITC reduces SSI is improved health of the child. In particular I find that exposure to a larger EITC does not reduce the likelihood a child reports a physical or cognitive impairment. Nor do I find any evidence that the primary channel through which the EITC reduces SSI participation is through increases in maternal labor supply, which may mechanically reduce the child's eligibility for SSI.

BIOGRAPHICAL SKETCH

Cassandra Benson grew up in Pocatello, ID with her five siblings: Scott, Chelsea, Chloe, Camille, and Rebecca. She earned her B.A. in Mathematical Economics from Colorado College in 2012. Following her undergraduate studies she worked for one year as the Technical/Statistical Coordinator for the Colorado College Department of Economics and Business. While at Colorado College Cassandra conducted research on fundraising campaigns of nationally ranked liberal arts colleges and student course taking behavior. In 2013, Cassandra started her graduate work at Cornell University where she earned her M.A. and Ph.D. in Economics in 2017 and 2019, respectively. At Cornell, Cassandra continued work on the Economics of Higher Education under the supervision of Ron Ehrenberg and began work on the Economics of Education under the supervision of Maria Fitzpatrick. Cassandra's current research focuses on federal and local policies that affect children with disabilities. Outside of academic work, Cassandra is an accomplished athlete. She ran the Boston Marathon in 2016 and 2017, was the first place finisher of the Cayuga Lake Triathlon in 2016, and was the first place finisher of the Greater Binghamton Marathon in 2015. Cassandra is excited to bike her first century, which will take place one week before graduation. After graduation Cassandra is thrilled to return to Colorado Springs as an Assistant Professor of Economics in the Department of Economics and Geosciences at the United States Air Force Academy.

for my family

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1.1 Introduction

Conventional wisdom holds that relatively older children are more mature and have garnered additional necessary skills compared to their younger counterparts; this maturity likely affects performance in the classroom. A positive association between student's relative age and early academic achievement has been document in the literature (Bedard and Dhuey, 2006; Elder and Lubotsky, 2009; Black, Devereux and Salvanes, 2011; Cook and Kang, 2016, and Dhuey, Figlio, Karbownik and Roth, 2017). Using administrative student level data and regression discontinuity methods, I corroborate earlier research indicating that in primary school the relatively youngest children in the classroom are more likely to be classified for special education compared to their older peers; in particular, children born directly before a school entry cutoff date are more likely to be classified with specific learnings disabilities, other health impairments, and speech impairments. I further corroborate findings that the youngest children in the classroom score below their older classmates on end of grade exams in both math and reading. Contrary to previous findings, I find the impact of relative age does not diminish during a child's academic tenure. Rather test score gaps and special education placement gaps persist through high school completion. Thus, I find evidence of a negative feedback loop in which the youngest children are placed on a lower track at the onset of their schooling from which they generally do not recover.

In the 2015-16 school year, approximately 6.6 million children between the ages of three and twenty-one received some form of special education services (NCES, 2016). While special education students comprise only 13% of all students, they receive about 18% of all funding for public schools (USDE, 2015). While advocates of special education view special education as an avenue in which to improve the well-being of highly-disadvantaged children, labeling a student as disabled may lower the expectations of teachers, parents, future employers, and children themselves. These lower expectations may place the child on a lower trajectory. For example, Carlana, 2018 finds that a teacher's expectation has a large impact on test score gaps.

This documented discrepancy in special education diagnoses at early ages for children born around an arbitrary cutoff date elicits further research into the long-term impacts of school cutoff dates on children's academic experiences. I expand the growing literature by testing how school cutoff dates affect the trajectory of children up through high school completion.

Beginning with Angrist and Krueger (1991) a series of articles have utilized the timing of one's birth to explore a myriad of outcomes including: academic achievement, graduation, earnings, and fertility (Black, Devereux and Salvanes, 2011; Bedard and Dhuey, 2006; Dobkin and Ferreira, 2010; McCrary and Royer, 2011). This burgeoning literature exploits state laws specifying the minimum age at which a child is eligible for kindergarten entry. While the short-term effects for relative youth, have been consistently documented as negative, the longer term effects are noisier. Elder and Lubotsky (2009) document that despite large test score gaps present at the beginning of kindergarten, the disparity in reading and math test scores dissipates between older and younger students by the time students reach 8th grade. However, Bedard and Dhuey (2006) find that test score gaps between older and younger classmates persist through 8th grade. I find no evidence that test score gaps dissipate with absolute age corroborating the findings of Bedard and Dhuey.

To test the effect of relative age in grade on special education in grades 3 through 12, I implement a regression discontinuity design (RD). I leverage rich administrative data from the state of North Carolina and document a long lasting negative impact of entering school as the youngest in the classroom. I find that the youngest children in the classroom are 1.75 percentage points (16.5%) more likely to receive special education services in grade three relative to their older classmates born directly after the school cutoff date. Importantly this effect does not dissipate with time. By eighth grade, I find that the youngest children in the classroom are 4.3 percentage points (34%) more likely to receive special education services relative to their older classmates. And by twelfth grade the youngest children are 3.8 percentage points (40%) more likely to receive special education services relative to their older classmates.

I corroborate Dhuey and Lipscomb (2010), where I find that the youngest children in the classroom are more likely to receive special education services for specific learning disabilities, other health impairments, and speech impediments. These three special education classifications tend to be more subjective than other classifications like vision or hearing impairments. I find that the youngest in the classroom are 1.2 percentage points (51%) more likely to be classified with an "other health impairment," relative to their classmates born the month after the cutoff date.[1] Similarly, the youngest are 0.3 percentage points (10%) more likely to be classified with a speech impairment and 1.97 percentage points (34%) more likely to be classified with a specific learning disability relative to their classmates born the month after the cutoff date. Importantly, I find no discontinuity in classification of blindness/deafness or autism for children born around the school cutoff date.

In addition to being placed in special education, I find that the youngest children in the classroom are more likely to repeat a grade. Children born the month before the cutoff date are 2.3 percentage points (28%) more likely to ever repeat a grade between grades 3 and 12. But this effect is even larger in early grades. For instance, children born the month before the cutoff date are 1.16 percentage points (73%) more likely to repeat 3rd grade, relative to their classmates born the month after the cutoff date.

Similar to Dhuey et al. (2017) I document that the youngest children perform worse on their math and reading end of grade exams. I find that children born the month before the cutoff date (the youngest in the classroom) score 0.12 standard deviations below their classmates born the month after the cutoff date on their end of grade 3 reading exam, and 0.10 standard deviations below their older counterparts on their end of grade 3 math exam. Similar to Bedard and Dhuey (2006) and contrary to Elder and Lubotsky (2009) I find that this negative test score gap persists through high school. On end of grade 8 exams, children born the month before the cutoff date score 0.10 standard deviations lower on reading and 0.09 standard deviations lower on math relative to their counterpart born the month after the cutoff date.

During high school children are tested on three subject exams: Algebra I, English II, and Biology. I find that the children born the month before the cutoff date score 0.15 standard deviations lower on their Algebra I test, 0.24 standard deviations lower on their English II test, and 0.16 standard deviations lower on their Biology exam. In addition to lower high school exam scores, I find that the youngest children tend to sit for their high school exams at a later grade; children born the month before the cutoff date complete their exams 0.5 grade levels later than

[1] Other health impairment (OHI) includes Attention Deficient Hyperactivity Disorder (ADHD) and Attention Deficient Disorder (ADD).

their classmates born the month after the cutoff date. These results suggest that children who enter as the youngest in their grade take longer to be test ready.

My analysis leverages rich administrative data from the North Carolina public school system. If individual characteristics associated with academic achievement are balanced at the cutoff date, the differences in outcomes can be interpreted as the causal effect of school entry eligibility on special education placement, test scores, and grade progression. I interpret these increases in child special education placement for children born just before the school cutoff date as intent-to-treat (ITT) effects of early eligibility to school entry for children on the margin of special education placement. My approach follows other studies using school entry eligibility cutoff dates to understand the intent-to-treat impact of relative age on school outcomes (e.g. Black, Devereux and Salvanes, 2011; Bedard and Dhuey, 2006; Dobkin and Ferreira, 2010; McCrary and Royer, 2011, Cook and Kang, 2016; Dhuey et al., 2017).

While Angrist and Krueger (1991) document a positive effect of being the youngest in the grade—higher likelihood of high school graduation, many recent works have elucidated a negative effect of being the youngest in the classroom. The youngest children score lower on end of grade exams (Dhuey et al., 2017), are more likely to be retained in grade (Elder and Lubotsky, 2009), and are more likely to be placed in special education for behavioral or learning disorders (Elder and Lubotsky (2009); Dhuey and Lipscomb, 2010).

Further, children born before a school entry cutoff date are more likely to be diagnosed with ADHD (Layton, Barnett, Hicks and Jena, 2018) and to be medicated with Ritalin for their ADHD (Elder, 2010). This higher prevalence of ADHD diagnosis and medication for relatively young children is troubling if these children are incorrectly diagnosed due to their relative immaturity rather than an underlying biological disorder. Given the fact that the true prevalence of ADHD remains unknown, these results likely reflect a combination of over-diagnoses for younger children and under-diagnoses for older children.

While Elder and Lubotsky (2009) find that the early advantage for older children dissipates over time, as evidenced by the diminished test score gap, the literature remains mixed on the existence of a continued advantage of relative-age-in-grade. Older children who score higher on standardized tests have been shown to enroll in college in higher rates (Bedard and Dhuey, 2006), to exhibit leadership characteristics in school (Dhuey and Lipscomb, 2010), and to earn higher adult wages (Fredriksoon and Ockert, 2005). However, more recent literature suggests mixed long-term outcomes for relative age effects. Black, Devereux and Salvanes (2011) find that the relatively oldest students perform worse on standardized test at age 18 and have lower earnings at age 30. But these relatively older children are less likely to have a teen pregnancy and less likely to suffer mental health problems. Cook and Kang (2016) find that the relatively oldest students in the class are less likely to exhibit delinquent behavior when aged 13 to 15, but they are substantially more likely to commit a felony by age 19. The attribute this increased likelihood to high school dropout.

I contribute to the literature by replicating and extending findings on the effect of school entry cutoff dates on academic performance, special education placement, and grade progression. Unlike earlier studies which were limited by data constraints, I am able to leverage the rich administrative data on individual students. This work was developed in parallel to Dhuey et al. (2017). Importantly, the findings presented here and those presented by Dhuey et al. are remarkably similar, suggesting that these findings generalizable outside the single state used for analysis. I find that children born the month before the cutoff date score approximately one-tenth to two-tenths of a standard deviation lower than their older counterparts on math and reading exams. I, further, corroborate the works of Dhuey and Lipscomb (2010) and show that relative-youth in the classroom affects special education primarily through classification of subjective impairments. Contrary to Dhuey and Lipscomb (2010), I find that relative youth in grade affects boys and girls equally. Whereas limited statistical power in studies by Dhuey and Lipscomb (2010) and Elder and Lubotsky (2009) were unable to find a persistent negative impact of relative youth in grade, I find these effects persist through 12th grade.

My estimates apply to children born directly around the school entry cutoff date, who may differ from children born in months further away from the cutoff date. For example, Buckles and Hungerman (2013) document maternal characteristics differ by child's season of birth. In particular, they find that children born in December and January are more likely to be born to an unwed mother with lower educational attainment. Because I use month of birth, rather than exact day of birth, seasonal differences may be a valid threat to the validity of this project if the children born in September are systematically different from children born in November. I test for differences in observable child characteristics for children born around the cutoff date and find quantitatively small differences in child characteristics for children born on either side of the cutoff date. Further, Dickert-Conlin and Elder (2010) show that parental characteristics are smooth through school cutoff dates. And to date, there is no evidence that parents are timing births around school cutoff dates.

This paper proceeds as follows. Section 1.2 provides background information on special education and school cutoff dates. Section 1.3 discusses the data and sample. Section 1.4 presents the empirical strategy. Section 1.5 outlines the change in special education diagnoses and test scores for children born around the cutoff date. And Section 1.6 concludes with a discussion.

1.2 Institutional Framework

1.2.1 Special Education in North Carolina

Under the Individual's with Disabilities Act of 1975, districts are mandated to provide all students with a free and appropriate education (FAPE). While IDEA provides general definitions of disability, local state agencies create their own precise diagnostic guidelines. Schools are charged with formally identifying children with disabilities and providing an individualized education program (IEP) (Cullen and Schmidt, 2011). Recent research has identified that teacher identification of disability can be idiosyncratic--driven by fiscal incentives (Cullen, 2003), school accountability incentives (Prenovitz, 2018), and subjective assessment of child maturity (Elder and Lubotsky, 2009; Dhuey and Lipscomb, 2010).

Most often, a teacher orchestrates a child's referral to special education. This occurs when the teacher notices that a student is struggling within her classroom. The school then conducts a disability evaluation, which assesses the student's abilities and needs (Prenovitz, 2018). If the child is found to need services in order to receive an appropriate education the child will have an IEP drafted. This IEP is written by a group of stakeholders that often consist of the child's parent, teacher, school psychologist, and school principal. The IEP will detail the specific services and support the child needs, as well as the setting these services will be delivered. A child's IEP is updated each year to reflect the expectations of the child, diagnoses of children are reviewed at minimum every three years, and removal of an IEP must be approved by all stakeholders (principal, psychologist, teacher, and parent).

A child can be diagnosed with one of thirteen disability classifications (e.g. autism, specific learning disability, visual impairment, etc.). In grade 3, the three most common disabilities are specific learning disability, which accounts for 37.55% of special education placements, speech impairments (35.6 %) and other health impairments (15.8 %). Autism makes up only 6.44% of special education placements, and visual and hearing impairments combined make up 1% of special education placements.

In North Carolina schools are allocated additional funds for each child with an IEP, up to a cap at 12.5 percent of all students identified as special education. In 2017, each child with an IEP would garner an additional $4,093 for the school regardless of which services and support the child's IEP identified as necessary to their education. However, once a school has identified 12.5 percent of the student body as special education, the school is ineligible to receive additional funding per student ((North Carolina Department of Public Instruction, 2017)).

1.2.2 School Entry Cutoff Dates

North Carolina, like the majority of states, adheres to a minimum age of enrollment into public kindergarten through its compulsory schooling law. Prior to the 2009-2010 school year, children who achieved the age of five on or before October 16 were eligible to enter school in the fall of the calendar year in which they turned five. In 2009 the law was amended, pushing the date forward from October 16 to August 31, thereby increasing the average age of children within each cohort. This statewide cutoff entry date creates a situation in which children born just before the cutoff date are essentially one year younger than their classmate who was born just after the statewide cutoff date.

For example, suppose there are three children: Child 1 is born on October 17, 2001; Child 2 is born on October 15, 2002; and Child 3 is born on October 17, 2002. Both Child 1 and Child 2 are first eligible to enter kindergarten in the 2007-08 School Year (SY). Child 1 enters school in September aged approximately 5 years and 11 months, whereas Child 2 enters school in September aged approximately 4 years and 11 months. Thus, despite nearly a full year difference in age, these children would be classmates. Children born just after the cutoff essentially wait an additional year before entering kindergarten as emphasized by the relationship between Child 2 and Child 3 who are nearly identical in biological age but enter school in different cohorts.

The actual distribution of students across grades is complicated by other features of the law. First, students attending private kindergarten do not adhere to the law, children migrating from other states are allowed to remain in the grade they transferred from, and children who choose not to enroll prior to first grade can circumvent the age cutoff date. An additional concern arises from parents choosing to "redshirt" their child by keeping them out of kindergarten for an additional year. In my data, I am not able to distinguish between each of these mechanisms that would leave a child in a different grade than the cutoff date would assign. Thus, my estimates will be interpreted as intention-to-treat (ITT) effects. Specifically, I estimate the effect of being eligible to enter school nearly a year earlier than a comparable control group on academic achievement, special education placement, and grade retention. My research leverages the variation in the relative-age of each child in their grade based on the school entry cutoff date, similar to prior work in this literature (Dobkin and Ferreira, 2010; Dhuey and Lipscomb, 2010; Cook and Kang, 2016; Elder and Lubotsky, 2009; Dhuey et al., 2017).

1.3 Data

I use restricted-access student-level data from the North Carolina Department of Public Instruction (NCDPI), provided by the North Carolina Education Research Data Center (NCERDC). The data covers all students in North Carolina's public school system with detailed information on children in grades 3 through 12. Since the 2004 school year, the data has included a summary file for children between the ages of 3 and 21 who receive special education services. My sample includes all individuals enrolled in North Carolina public schools between 2004 and 2014. There are 2,284,675 unique child observations with 1,118,069 children appearing in third grade entry and 804,995 appearing in 12th grade.

There are several limitations to this data: first, student date of birth is recorded as month of birth, rather than exact date of birth; second, I cannot follow students who chose to leave the public school system. If the group of students leaving the public school system were systematically different from those initially enrolled, and this difference was driven by the relative age of the child, my estimates would be biased. To address the former concern, I drop all October births facing an October 16 cutoff date. This is because October births represent both the oldest and youngest students in their grade. To address the second concern, I look for differences in the likelihood a child leaves NC public schools prior to graduation for those born around the cutoff date. I find that the youngest children are 1.7 percentage points (8%) more likely to leave the public school system relative to the oldest in the classroom. As a robustness check, I limit analysis to only the sample of individuals who remain enrolled in the NC public school system through the last year of the data or until graduation. These results indicate that differential attrition does not drive my findings; my estimates are robust to excluding individuals who leave the public school system. Despite the limitations, this administrative data provides more finely detailed observations than previous research utilizing smaller national panel surveys.

To construct the main analysis sample, I combine the masterbuild file for years 2004 through 2014 with the exceptionality file. Data is constructed such that each observation represents a unique child. I construct special education outcomes first as an indicator for whether child i received special education services in grade g, where $g \in [3, 12]$, and second as indicators for receiving services for each of the following disability classifications: autism, visual or hearing impairments, learning disabilities, other health impairments, or speech impairments. Grade repetition is first created as an indicator for ever repeating any grade. Similarly, to special education, grade repetition is further disaggregated to indicators for whether the child repeated each grade (grade 3 through 12) separately.[2] End of Grade (EOG) and End of Course (EOC) test scores are standardized to have mean of zero and a standard deviation of one for each test-grade-year cycle. This means that effect sizes will be interpreted in standard deviation units.

Table 1.1 displays sample means for the main analysis sample of North Carolina public school students. Column 1 includes the full sample of all children. This is disaggregated to children born 6 months after the cutoff (the oldest in the classroom) in Column 2 and those born 6 months before the cutoff date (the youngest in the classroom) in Column 3. Children born within 6 months prior to the cutoff date are approximately age 9 at their end of third grade exam, whereas children born within 6 months after the cutoff date are approximately age 9.5 at the time of their third grade exam. Approximately 13 percent of all students receive special education services in grade three and this falls to 10 percent of all students by grade 12.

Seven percent of students receive special education services for a specific learning disability,

[2] Approximately 10 percent of children will repeat a grade between grades 1 and 12. Children are most often retained in 9th grade, which accounts for 35% of all grade retentions. Third grade is the 3rd most common grade to be retained in after grade 9 and grade 10.

Table 1.1: Summary Statistics

Variable	All Mean	Oldest Mean	Youngest Mean
Age at Grade 3 Test	9.31	9.46	9.14
Special Education			
Grade 3	0.14	0.13	0.14
Grade 4	0.15	0.14	0.16
Grade 5	0.15	0.14	0.15
Grade 6	0.14	0.13	0.14
Grade 7	0.13	0.13	0.14
Grade 8	0.13	0.12	0.13
Grade 9	0.12	0.12	0.13
Grade 10	0.11	0.10	0.11
Grade 11	0.10	0.10	0.11
Grade 12	0.10	0.09	0.10
Type of Impairment			
Other Health Impairment	0.03	0.03	0.03
Speech Impairment	0.03	0.03	0.03
Learning Disability	0.07	0.06	0.07
Blind or Deaf	0.00	0.00	0.00
Autism	0.01	0.01	0.01
Read Grade 3	0.01	0.03	-0.02
Read Grade 8	0.01	0.02	0.00
English II	0.00	0.03	-0.04
Math Grade 3	0.01	0.03	-0.01
Math Grade 8	0.01	0.02	0.00
Algebra I	0.00	0.02	-0.02
Biology	0.00	0.02	-0.03
Grade Algebra I Taken	10.20	10.19	10.21
Grade Biology Taken	10.76	10.76	10.77
Grade English II Taken	10.28	10.28	10.28
Repeated a Grade	0.09	0.09	0.10
Left Sample Early	0.24	0.24	0.24
Male	0.51	0.51	0.51
White	0.54	0.54	0.54
Black	0.27	0.28	0.27
Hispanic	0.11	0.11	0.11
Free/Reduced Lunch	0.53	0.53	0.52
English Language Program	0.15	0.15	0.16
N	1,117,538	601,118	516,420

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12.

three percent receive special education services for speech impediments and other health impairments, respectively. Less than one percent of students receive special education for a hearing or visual impairment, and one percent of students receive special education for autism. Most children report completing their high school exams in Algebra I, English II, and Biology in grade 10. Approximately 9 percent of all students will repeat a grade at some point, and 24 percent of the sample is flagged as departing the north Carolina public school system prior to graduation. Approximately 50 percent of the sample is male, 54 percent identify as non-Hispanic white, 28 percent identify as non-Hispanic black, and 11 percent identify as Hispanic. Fifty-three percent of the sample is classified as free or reduced price lunch for at least one year during the sample period. And 15 percent are referred to the English Language Program at some point during their academic career. Importantly, special education classification, test scores, and student characteristics do not show any difference in means around the cutoff date.

This project relies on a data from a single state, North Carolina. In Appendix Figure A.2, I show that North Carolina is not dissimilar from the US average. Approximately 13 percent of children in North Carolina receive special education services, this is similar to the national average. Further, North Carolina children are equally likely to live below the poverty level, identify as white, have a mother with a high school diploma or less or live with a single mother. However, children in North Carolina are less likely to be non-Hispanic black and more likely to be Hispanic, relative to the US average. In general, North Carolina is a large state, that demographically resembles the national average; this makes North Carolina an appropriate setting to examine school entry cutoff dates and special education placement. The similarity of my estimates to those of Dhuey et al. (2017) using Florida student level data provides additional evidence that North Carolina is not unique setting to study relative-age-in-grade effects.

1.4 Empirical Strategy

To estimate the effect of relative age on special education placement, grade retention, and test scores I exploit the sharp discontinuity in eligibility to enter kindergarten between children born just before and just after the school entry cutoff date in a regression discontinuity design (RD). The variation in school entry eligibility and subsequent variation in relative-age-in-grade enables an estimate of the causal effect of school entry eligibility on children's special education and academic outcomes.

This strategy assumes that other characteristics associated with special education placement, grade retention, and test scores evolve smoothly through the cutoff date. Restated, the underlying assumption is that there are no other differences among children born just before and just after the school entry cutoff date. Date of birth is essentially random, therefore, children born around the cutoff date should not exhibit observable differences other than that one child was eligible to enter kindergarten and one child was ineligible to enter until the following year.

This assumption relies on parents not manipulating the date of birth around the school entry cutoff date. Dickert-Conlin and Elder (2010) find no systematic differences in maternal characteristics or child infant health outcomes around school entry eligibility cutoff dates. Their results suggest that in the neighborhood around a school entry eligibility cutoff date, date of birth is essentially random. As is standard with regression discontinuity methods, I test this underlying assumption by looking for discrete differences in characteristics of children born around the cutoff date. If this underlying assumption holds, the outcomes of children born just after the cutoff date (those ineligible to enter school) provide reasonable counterfactual outcomes to the children born just before the cutoff date.

To implement this strategy, I define the running variable as the distance between an individual's month of birth and the statewide school entry cutoff month as shown in the following equation:

$$relative\ month_i = month\ of\ entry\ cutoff - month\ of\ birth_i \tag{1.1}$$

where $month\ of\ birth_i$ is the child's month of birth (with January= 1 and December =12) of individual i, and $month\ of\ entry\ cutoff$ is the statewide school entry cutoff month (October=10). By this definition any child with $relative\ month < 0$ is among the oldest in the classroom and any child with $relative\ month > 0$ is among the youngest in the classroom. Thus, an individual born in September will have a relative month of 1 and an individual born in November will have a relative month of negative 1.[3] I then compare differences in the likelihood a child receives special education services, the likelihood a child is retained in their grade, and end of grade test scores

[3]October births would take a relative month of 0 but have been dropped from analysis due to the October 16 cutoff date.

between the youngest and oldest within each grade and school. To quantify the discontinuity more precisely, I estimate regressions of the following form:

$$Y_{isc}^{g} = \alpha + \delta_1 Youngest_i + \delta_2 relative\ month_i + \delta_3 Youngest_i * relative\ month_i + \pi_c + \theta_s + \gamma X_i + \varepsilon_{isc} \quad (1.2)$$

where Y^g is the outcome of interest (e.g. Special education placement in grade $g \in [3,12]$, retained in grade g, math/reading score in grade g) for individual i, in school s, born in year c. *Youngest* is an indicator for having a birth month before the school cutoff month (having a $relative\ month > 0$). I include birth year and school fixed effects represented by π_c and θ_s, respectively. Lastly, X_i represents a vector of individual characteristics which include the child's race, gender, and whether they are eligible for free and reduced price lunch. Equation 1.2 specifies the local linear regression, which allows the relationship between relative age and child outcomes to vary on either side of the discontinuity.[4] The coefficient of interest, δ_1, is the size of the discontinuity. That is, δ_1, measures the weighted average treatment effect where the weights are determined by the probability of being near the cutoff date or where $relative\ month = 0$ (Lee, 2008).

The RD estimate represents the difference in special education placement, grade retention, and test scores for children who are essentially one year younger than their classmates who were born the month after the cutoff date. The estimates reported in this project are derived from the local linear specification using a bandwidth of 6 months. Standard errors are clustered at the school. However, I show that my estimates are robust to bandwidth selection, specification choice, and different assumptions about the correlation of standard errors.

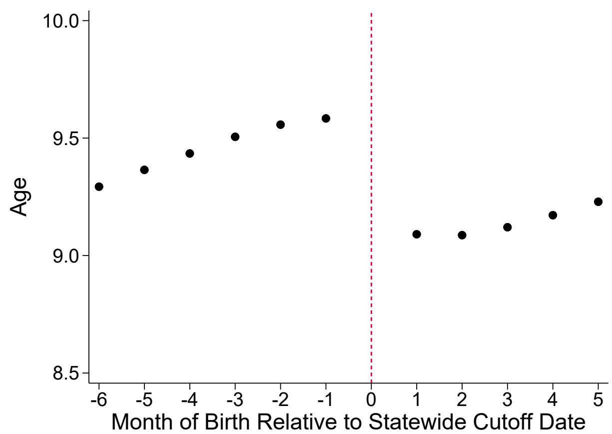
1.5 Regression Discontinuity Estimates

To begin analysis, I corroborate earlier research that school entry cutoff dates affect the age of children within the classroom. Figure 1.1 plots the average age of children at the time of their end-of-grade 3 test by their month of birth relative to the school entry cutoff month. Children

[4]Estimates are not sensitive to using the local linear specification. Estimates using higher order polynomials are qualitatively similar and presented in table A.1.

born in September have a relative month of 1, whereas children born in November have a relative month of -1. As can be seen, children born in November are about half a year older at the time of their end-of-grade 3 exam. The fact that the difference in age at the time of test is not exactly 1 year reflects the fact that some children are retained in grade and/or parents choose to red-shirt their child. However, there is a clear discontinuity in the average age of children around the school cutoff date. I next test the underlying assumption of the model, that characteristics of individuals evolve smoothly through the cutoff month.

Figure 1.1: Age of Child at End of Grade 3 Exam, By Month of Birth Relative to Statewide Cutoff Date

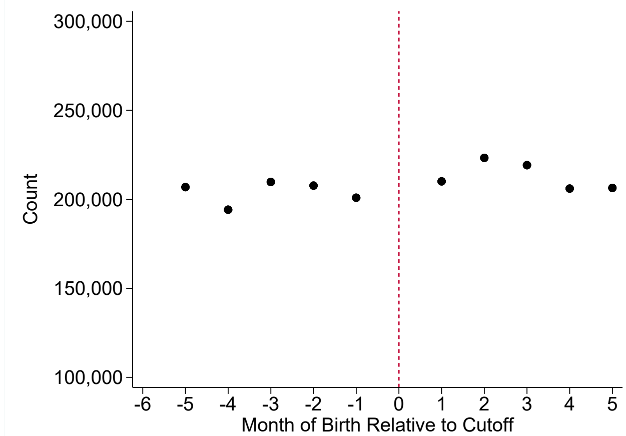


Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended third grade in a North Carolina public school. All October births have been dropped from the sample. Age of child is calculated as the biological age of the child in months relative to a June test date. Children born in November have a relative month of -1 and are the oldest in the classroom. Children born in September have a relative month of 1 and are the youngest in the classroom.

1.5.1 Balance of Covariates

Previous work by Buckles and Hungerman (2013) argued that season of birth may be a poor instrument for educational attainment because women who give birth in winter are more likely to be unmarried and/or teenage mothers. Thus, maternal characteristics may explain the differences one finds in educational outcomes due to season of birth instrumental variables. In this setting, in which I only observe month of birth, rather than exact day of birth, differences in child characteristics is a valid concern. I first explore whether there is bunching around the October cutoff

Figure 1.2: Total Births, By Month of Birth Relative to Statewide Cutoff Date



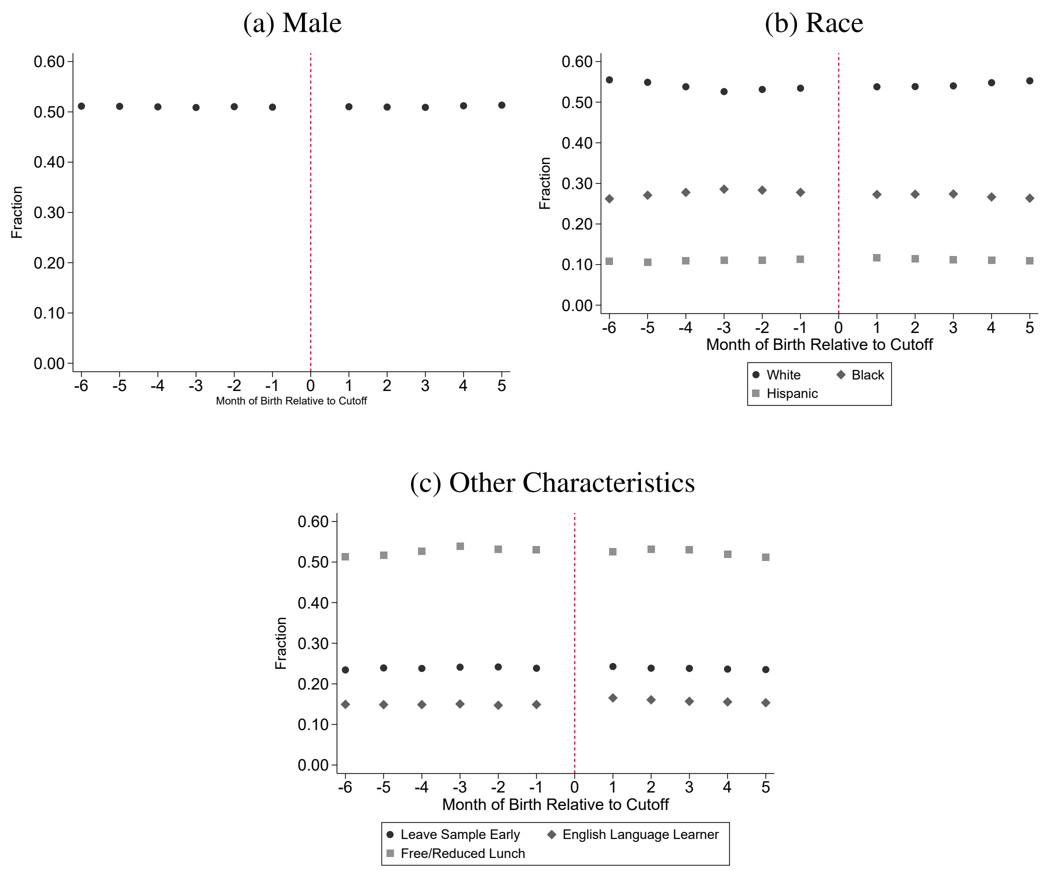
Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any grade 3 through 12. All October births have been dropped from the sample.

month. Figure 1.2 plots the total number births by month of birth relative to the cutoff month. While there are fewer births in November, relative to September, the difference in births does not appear large. Further, the distributions of births by month closely follow the national distribution of births, in which no other state uses an October school entry cutoff date (see Appendix Figure A.1). Therefore, it does not appear that parents are timing births around the cutoff month.

I next present graphical and tabular evidence indicating that child characteristics are smooth through the cutoff month. In Figure 1.3, I plot the fraction of persons who identify with the following characteristics: male, non-Hispanic white, non-Hispanic black, Hispanic, free and reduced price lunch eligible, English Language Program participants, and those who are flagged as having left the public school system prior to graduation by their month of birth relative to the school cutoff month. Panel (a) shows that there is no discontinuity in the fraction of children who are male. Panel (b) shows that there are no discontinuities at the cutoff date by child race. And Panel (c) shows that children on either side of the cutoff data do not appear any more likely to qualify for free/reduced price lunch, be assigned to the English Language Program, or leave the sample early.

Lastly, using each observable characteristic as the dependent variable, I estimate models of Equation 1.2 to test for discontinuities in characteristics of children born around the cutoff month. Table 1.2 presents estimates of δ_1 from Equation 1.2, where I find small, yet statistically

Figure 1.3: Fraction of Births by Relative Month, by Observable Characteristics



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any grade 3 through 12. All October births have been dropped from the sample. Male is an indicator that the child is a biological male, white is an indicator for non-Hispanic white, black is an indicator for non-Hispanic black, Hispanic is an indicator for Hispanic, Other has been omitted from the graph. Leave Sample Early is an indicator for the child left the NC public school system prior to graduation. English Language Learner is an indicator that the child participates in the English Language Program at any time between grades 3 and 12. Free/Reduced Lunch is an indicator for the child was eligible for free or reduced price lunch at any time between grades 3 through 12.

significant, differences for children born around the cutoff month. Children born the month before the cutoff date are 0.49 percentage points (1.8%) more likely to be black, 0.21 percentage points (1.8%) more likely to be Hispanic, 0.49 percentage points (0.9%) less likely to be white, and 0.29 percentage points (4%) less likely to identify as another race. These differences in child ethnicity around the cutoff date, while statistically significant, are small in magnitude. I also find that children born the month before the cutoff date are 1.1 percentage points (6.5%) more likely to participate in the English Language Program.[5]

Table 1.2: Regression Discontinuity Estimates of Being Youngest in Classroom on Covariates

	Black	Hispanic	White	Other Race	Ever FRL	Ever ELP
	(1)	(2)	(3)	(4)	(5)	(6)
Youngest	-0.0049***	0.0021*	0.0049***	-0.0029***	0.0011	0.0112***
	(0.0014)	(0.0011)	(0.0015)	(0.0009)	(0.0017)	(0.0016)
Dep. Mean	0.2737	0.1114	0.5411	0.0702	0.5121	0.1669
N	1,782,480	1,782,480	1,782,480	1,782,480	1,782,480	1,782,480

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. FRL is Free/Reduced price lunch eligible. ELP is English Language Program for students learning English as a second language. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school and are estimated separately by grade. Standard errors are clustered at the school and are reported in parentheses. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Importantly, I find no evidence that children born the month before the cutoff date are more likely to qualify as free and reduced price lunch. The point estimate is small and not statistically significant. Therefore, family socioeconomic status does not appear to predict child month of birth, in this setting. Taken together, the density plots, descriptive statistics, and regression discontinuity estimates presented in Table 1.2 suggest that the underlying assumptions of the model are satisfied. Month of birth is essentially random, and individuals born the month after the cutoff date provide a valid counterfactual for the children born the month before the cutoff date. Thus, differences in special education placement, grade retention, and test scores can be attributed to the child's immaturity in the classroom.

[5] English Language Program participation may be an outcome, rather than a characteristic. Similar to special education, relatively immature students are more likely to be referred for ELP for additional support.

1.5.2 Graphical Evidence

To graphically view the increase in special education placement for children born near the cutoff date, I plot the fraction of individuals with a documented IEP by month of birth relative to the statewide cutoff month. This is shown in Figure 1.4, where the running variable is month of birth relative to the cutoff month. As can be seen in panel (a) there is a large increase in the fraction of children receiving special education services at the cutoff month. Approximately 14.7 percent of children born in September receive special education, whereas only 12.4 percent of children born in November receive special education services. Panel (b) shows that the discontinuity is driven by changes in special education placement for Learning Disabilities, Other Health Impairments, and Speech impairments. There is no visual discontinuity in diagnoses for autism or visual/hearing impairments. The fact that there are no discontinuities in these "non-malleable" impairments provides validity for the underlying assumption that children born in September are not systematically different from children born in November.

Turning to student test scores, Figure 1.5 plots the average math and reading z-score by month of birth relative to the cutoff month. All six panels show that the youngest children in the classroom perform worse on math and reading exams in grade 3 and grade 8. Further, I find large, and statistically significant, discontinuities in Algebra I and English II exam scores. Children born the month before the cutoff date perform approximately one-tenth of a standard deviation worse on end-of-grade exams, regardless of grade level. Lastly, Figure 1.6 plots the fraction of children who are ever retained in a grade between grades 3 and 12. While approximately 10 percent of all children are retained in a grade at some point during primary or secondary school, the children born the month before the school cutoff date are more likely to repeat a grade than their older counterparts born the month after the cutoff date.

1.5.3 Reduced-Form Estimates

To confirm the visual evidence of differences in special education placement, I present estimates of equation (1.2) in Table 1.3. Each cell contains results from a separate regression. Each specification is a local linear regression with a bandwidth of 6 months, standard errors are clustered at the school, and dependent means are presented below the standard error. These estimates are unweighted, and thus, represent the local average treatment effect for the sampled population. I find that children born the month before the cutoff date are 0.59 years younger than their classmates born the month after the cutoff date at the time of their end-of-grade 3 exam. Thus, school cutoff dates do affect the age of children within the classroom.

Table 1.3: Regression Discontinuity Estimates of Being Youngest in Classroom on Grade 3 Outcomes

	Age at Grade 3 Exam (1)	Grade 3 Math Z-Score (2)	Grade 3 Read Z-Score (3)	Receives SpEd in Grade 3 (4)	Repeated Grade 3 (5)
Youngest	-0.5952***	-0.1009***	-0.1244***	0.0175***	0.0116***
	(0.0030)	(0.0041)	(0.0041)	(0.0015)	(0.0006)
Dep. Mean	9.4511	0.0596	0.0638	0.1062	0.0158
N	1,083,943	1,075,424	1,074,108	1,083,946	1,083,946

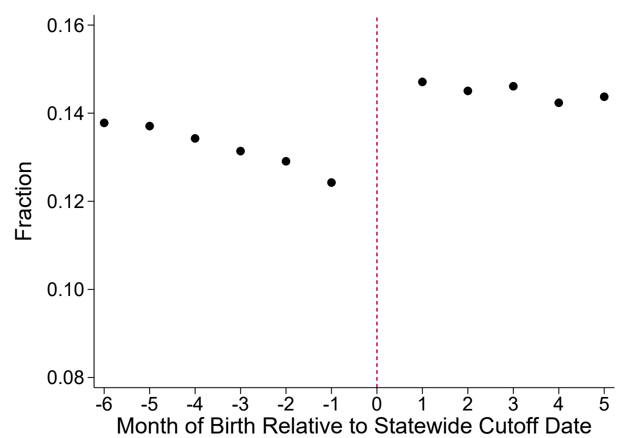
Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Special education includes thirteen separate classifications of disabilities. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. Repeated grade is an indicator that the child was recorded in third grade in two consecutive years. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Further, children born the month before the school cutoff date perform 0.10 standard deviations lower on math and 0.12 standard deviations lower on reading than their counterparts born the month after the cutoff date. These estimates are slightly smaller than those found by Dhuey et al. (2017), who estimate the youngest children score 0.2 standard deviations lower on math and reading. One potential explanation for the discrepancy between these two sets of estimates is the October 16 cutoff date in North Carolina. Dhuey et al. use Florida student-level data. While they are similarly limited to child month of birth, the statewide cutoff date in Florida is September 1. Therefore, Dhuey et al. have a clean break between August births and September births. Whereas, in this setting, I have dropped both the oldest and youngest children from the sample because I cannot distinguish between the two children as they share the same birth month and face the same October 16 cutoff date. In addition to higher rates of special education, I find that the children born the month before the school cutoff date are 1.16 percentage points (73%)

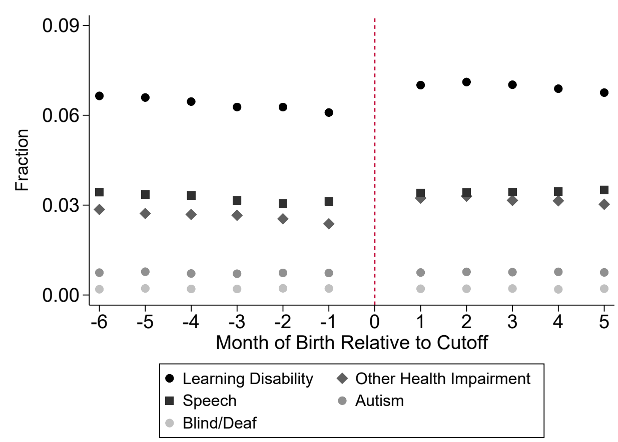
more likely to repeat the third grade relative to their older classmates born the month after the cutoff date (column 5).

Figure 1.4: Special Education by Month of Birth Relative to Cutoff, Grade 3

(a) Special Education for Any Reason



(b) Special Education by Type of Impairment



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3. All October births have been dropped from the sample. Special education includes thirteen separate classifications of disabilities. Panel (b) disaggregates all of special education diagnoses by classifications. Not all learning disabilities are shown. Visual and Hearing impairments have been aggregated and label as blind/deaf.

Turning to the main question of interest in this project, I find that the children born the month before the school cutoff date are 1.75 percentage points (16%) more likely to receive special education services relative to their classmates born one month after the school cutoff month. Table 1.4 disaggregates special education placement by five types of impairments. The first three

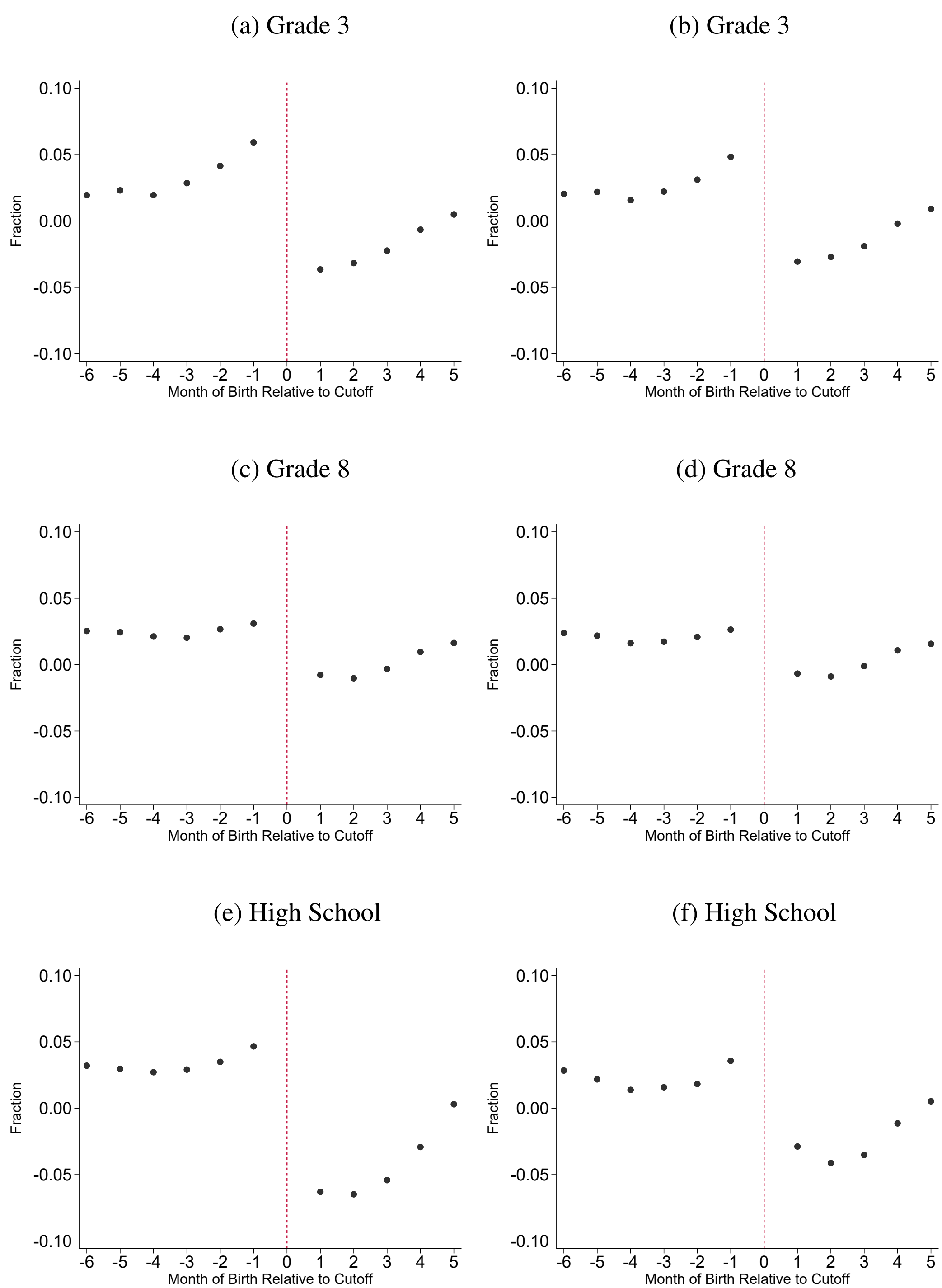
Figure 1.5: Math and Reading Z-Scores by Month of Birth Relative to State Cutoff, by Grade

READING | MATH

(a) Grade 3 | (b) Grade 3

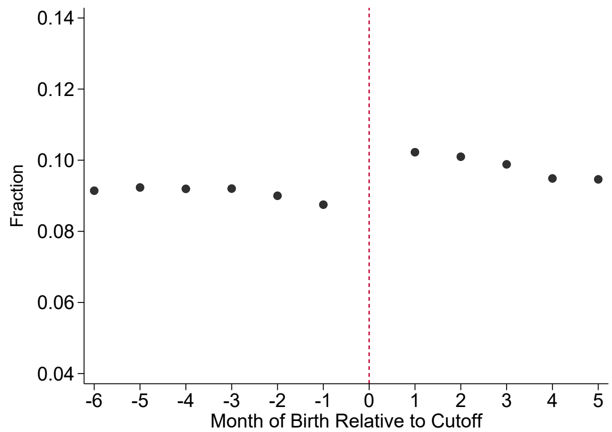
(c) Grade 8 | (d) Grade 8

(e) High School | (f) High School



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades between grade 3 and 12. All October births have been dropped from the sample. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one.

Figure 1.6: Fraction of Children Retained in Any Grade, by Month of Birth Relative to State Cutoff Date



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades between grade 3 and 12. All October births have been dropped from the sample. Grade repetition is defined as having ever been retained in a grade (grade 3 through 12). Most children are retained in grade 9, grade 10, or grade 3.

(learning disability, other health impairment, and speech) are expected to be more malleable and subjective to teacher identification of student impairment. Whereas the latter two (blind/deaf and autism) are less malleable and should not depend on the child's relative maturity within the classroom. I find that children born the month before the cutoff date are 0.88 percentage points (30%) more likely to be diagnosed with a learning disability relative to their older classmates born the month after the cutoff date. The youngest children are also 0.65 percentage points (57%) more likely to be diagnosed as other health impaired, which includes ADHD/ADD, relative to their older classmates. And the youngest children are 0.49 percentage points (26%) more likely to be diagnosed with a speech impairment relative to their older classmates born the month after the cutoff date. Importantly, I find no discontinuity in the likelihood that children born the month before the cutoff date are diagnosed with a visual or hearing impairment. However, in third grade I find that the children born the month before the school cutoff date are 0.07 percentage points (17%) more likely to be diagnosed with autism relative to their older peers. However, this may be a false positive effect as I find that the positive coefficient on autism classifications does not appear when I test whether a child has ever been diagnosed with autism.

Table 1.5 reports estimates of whether a child is ever diagnosed for special education by

Table 1.4: Regression Discontinuity Estimates of Being Youngest in Classroom on Special Education Classification in Grade 3

	Learning Disability (1)	Other Health Impairment (2)	Speech (3)	Blind/Deaf (4)	Autism (5)
Youngest	0.0088***	0.0065***	0.0049***	0.0001	0.0007**
	(0.0008)	(0.0005)	(0.0006)	(0.0001)	(0.0003)
Dep. Mean	0.0296	0.0113	0.0185	0.0010	0.0040
N	1,083,946	1,083,946	1,083,946	1,083,946	1,083,946

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Special education is disaggregated to some classifications, not all classifications are included. Visual and hearing impairments have been aggregated together in the variable Blind/Deaf. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

classification type. Outcomes are indicators for the individual was ever diagnosed between grade 3 and grade 12. I find that the children born the month before the school cutoff date are 1.97 percentage points (33%) more likely to be diagnosed with a learning disability, 1.2 percentage points (52%) more likely to be diagnosed with OHI, and 0.3 percentage points (13%) more likely to be classified as speech impaired relative to children born the month after the cutoff date. I find no increase in the likelihood that a child is ever diagnosed with a visual/hearing impairment or autism.

To test whether this relative age effect dissipates with time, I estimate the effect of relative youth for each grade separately. Figure 1.7 plots the estimated coefficient of interest from equation 1.2 for receipt of special education services. As can be seen in the top left panel children born the month before the cutoff date are more likely to receive special education services at all grades, relative to their classmates born the month after the cutoff date. In grade 12, children born the month before the cutoff date are 3.84 percentage points (42%) more likely to receive special educations services relative to their peers born the month after the cutoff date. This suggests that special education is an absorbing state, in which children do not leave the special education program. Importantly, when special education services are disaggregated to the diagnosis classification, I find heterogeneity in the likelihood children leave special education prior

Table 1.5: Regression Discontinuity Estimates of Being Youngest in Classroom on Ever being Diagnosed in the Following Special Education Classification

	Learning Disability (1)	Other Health Impairment (2)	Speech (3)	Blind/Deaf (4)	Autism (5)
Youngest	0.0197***	0.0122***	0.0038***	0.0001	0.0001
	(0.0009)	(0.0006)	(0.0006)	(0.0001)	(0.0003)
Dep. Mean	0.0585	0.0234	0.0280	0.0018	0.0059
N	1,782,480	1,782,480	1,782,480	1,782,480	1,782,480

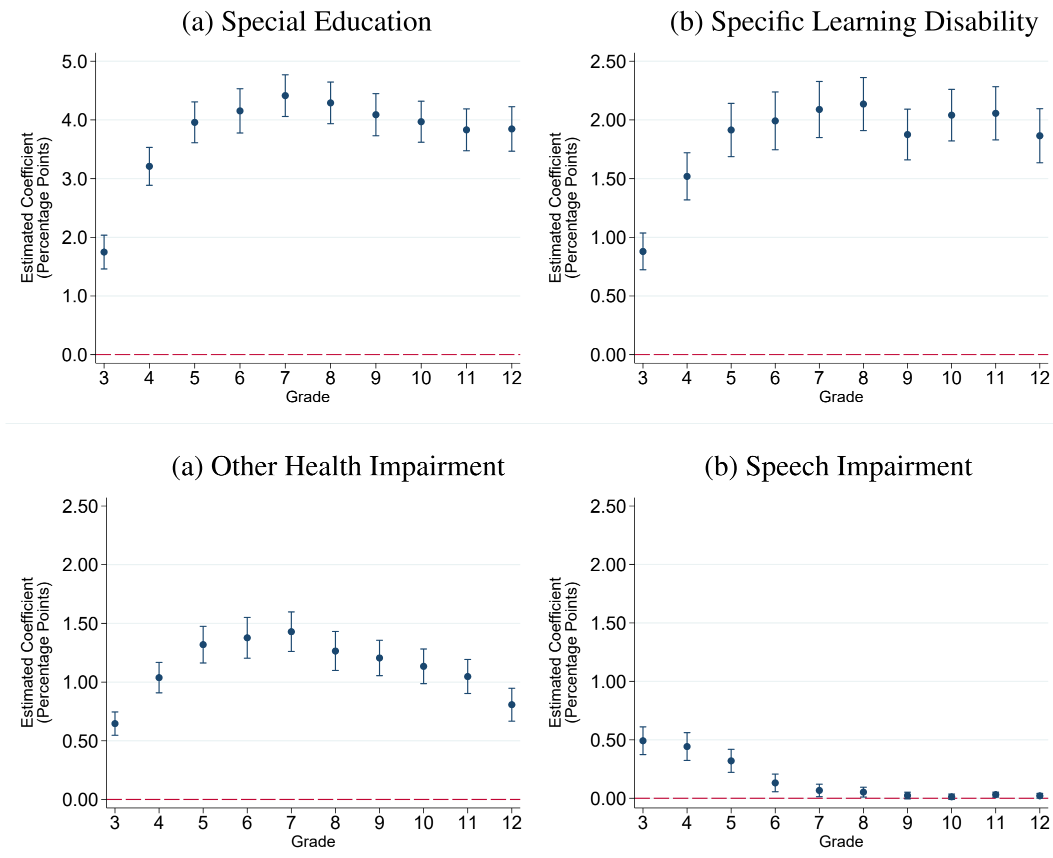
Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades between grade 3 and grade 12. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Special education is disaggregated to some classifications, not all classifications are included. Visual and hearing impairments have been aggregated together in the variable Blind/Deaf. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

to grade 12.

Figure 1.7 shows that children diagnosed with a specific learning disability are unlikely to leave special education by grade 12. In grade 12, children born the month before the cutoff date are still 1.8 percentage points (55%) more likely to be diagnosed with a specific learning disability relative to their counterparts born the month after the cutoff date. However, children diagnosed with other health impairments or speech impediments do leave special education. By grade 7, children born the month before the cutoff date are no more likely to be classified with a speech impairment relative to their classmates born the month after the cutoff date.

Having shown that children perform worse on their end-of-grade 3 exams, I turn to testing whether this test score gap diminishes with time, as suggested by the prior literature. Figure 1.8 plots the estimated coefficient from equation (1.2) for math and reading EOG scores between grades 3 and 8. I find that the children born the month before the cutoff date consistently perform approximately one-tenth of a standard deviation lower on math and reading tests relative to their peers born the month after the cutoff date. While, these results contradict the findings of Elder and Lubotsky (2009), who found that achievement gaps between the relatively old and relatively young fade after school entry, they corroborate the findings by Dhuey et al. (2017).

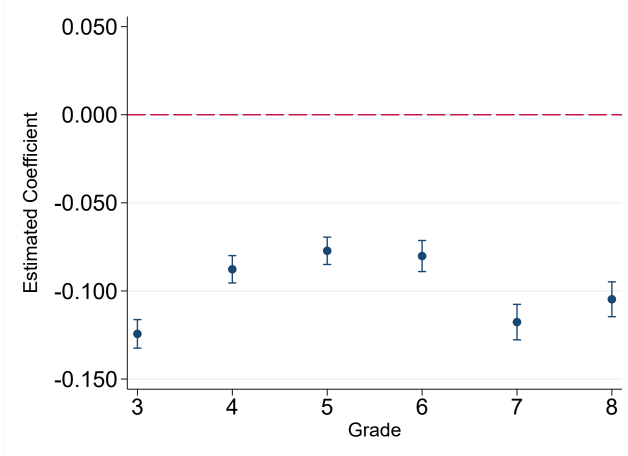
Figure 1.7: Regression Discontinuity Estimates of the Effect of School Cutoff Dates on Special Education, by Grade and Type of Impairment



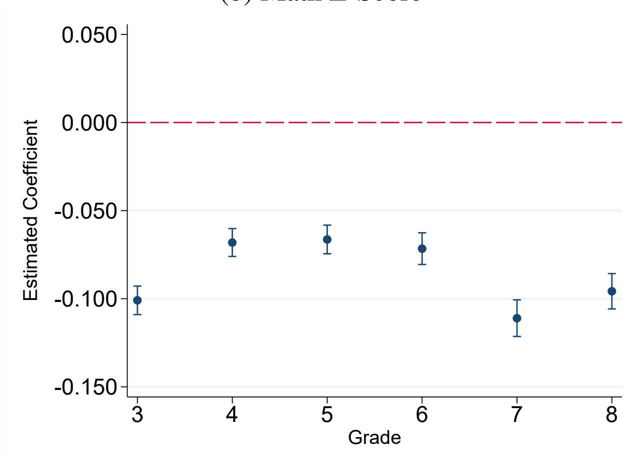
Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. Special education includes thirteen separate classifications of disabilities. Special education has been disaggregated into three classifications that are subjective. These figures show the coefficient and 95 percent confidence interval for special education by type of diagnosis at a given grade. The local linear regressions are estimated using robust standard errors clustered at the school with a bandwidth of 6 months. The effect of the school entry cutoff date persists through 12th grade for overall special education and specific learning disabilities.

Figure 1.8: Regression Discontinuity Estimates of the Effect of School Cutoff Dates on End-of-Grade Math and Reading Scores, by Grade

(a) Reading Z-Score



(b) Math Z-Score



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. These figures show the coefficient and 95 percent confidence interval for math and reading end-of-grade exams at a given grade. The local linear regressions are estimated using robust standard errors clustered at the school with a bandwidth of 6 months. The effect of the school entry cutoff date persists through 8th grade.

Finally, Table 1.6 presents estimates of δ_1 for End-of-Course (EOC) tests in Algebra I, English II, and Biology. These EOC exams are taken in high school typically between grades 9 and 11. I find that children born the month before the cutoff date perform one-sixth of a standard deviation lower on Algebra and Biology and one-fourth of a standard deviation lower on English relative to children born the month after the cutoff date. To some extent, students only sit for an end-of-course exam when they have completed the course and are test-ready. The youngest children who may progress more slowly through primary and secondary school may therefore sit for their end of course exams at a later grade. I find that children born the month before the school entry cutoff complete their end of course exam 0.5 grade levels later than their peers who were born the month after the cutoff date.

Table 1.6: Regression Discontinuity Estimates of Being Youngest in Classroom on High School Tests

	Algebra		English		Biology	
	Z-Score (1)	Grade Taken (2)	Z-Score (3)	Grade Taken (4)	Z-Score (5)	Grade Taken (6)
Youngest	-0.1533***	0.4560***	-0.2461***	0.5100***	-0.1680***	0.4885***
	(0.0080)	(0.0079)	(0.0078)	(0.0061)	(0.0097)	(0.0064)
Dep. Mean	0.0223	10.1860	0.0332	10.2794	0.0231	10.7594
N	320,499	320,499	316,708	316,708	222,240	222,240

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any grade between grade 9 and 12. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. Grade taken is a grade level between 9 and 12 in which the child completed their End of Course exam. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

1.5.4 Heterogeneity

To explore how relative youth in grade differentially affects various demographic groups, I disaggregate by gender and race. Figure 1.9 plots the estimated coefficients by subsample. Panel (a) shows that relative-youth-in-grade affects both boys and girls; prior to tenth grade I cannot rule out the possibility that the point estimates for boys and girls are the same. While the point estimates between boys and girls are similar, given the base rate of diagnoses the effect sizes are much larger for girls. For example, in grade 3, boys are 1.7 percentage points (12%) more likely to receive special education relative to their older male peers, but girls are 1.8 percentage points (26%) more likely to receive special education services relative to their older female counterparts. By 12th grade, the boys born just before the cutoff date are 4.4 percentage points (35%) more likely to receive special education services relative to their older male peers; while the youngest girls are 2.9 percentage points (47%) more likely to receive special education services relative to their older female peers. These results corroborate the findings of Dhuey and Lipscomb (2010) showing that after grade three boys and girls are both negatively affected by relative youth in their classroom setting. Contrary to Dhuey and Lipscomb, effect sizes indicate that the youngest girls face the largest immaturity penalty relative to their peers.

Panel (b) shows that relative youth differentially affects children of color. In third grade, the youngest black students are 2.8 percentage points (24%) more likely to receive special education relative to their older peers born the month after the cutoff date. Similarly, the youngest Hispanic students are 1.6 percentage points (19%) more likely to receive special education services relative to their older counterparts born the month after the cutoff date. Whereas, the youngest white students are only 1.3 percentage points (11%) more likely to receive special education services relative to their older classmates born the month after the cutoff date. The black-white gap identified here, does not persists through 12th grade. In grade 12 black students born the month before the cutoff date are 5.2 percentage points (44%) more likely to receive special education services relative to their older peers, while white students are 3.6 percentage points (45%) more likely to receive special education services relative to their older peers born the month after the cutoff date. This reflects the fact that black students make up a larger fraction of the special education placements. Lastly, I find that by 11th grade Hispanic students born the month before the cutoff date are no more likely to receive special education relative to their older counterparts born the month after the cutoff date.

Figure 1.10 presents RD estimates of test scores disaggregated by gender and race. I find that the youngest boys and girls are equally penalized during end-of-grade math and reading exams. Point estimates between boys and girls are not statistically different from one another after grade 3. However, I do find that black students face the largest test score penalty. Black students born the month before the cutoff date score 0.15 standard deviations lower on their end-of-grade 3 math exam relative to their peers born the month after the cutoff date. Compare this to the youngest white students, who score 0.06 standard deviations below their peers, and Hispanic students, who score 0.11 standard deviations below their older peers on their end-of-grade 3 math exam. Test score gaps persist for black, Hispanic, and white students through 8th grade. Results are similar for end-of-grade reading scores.

1.5.5 Robustness

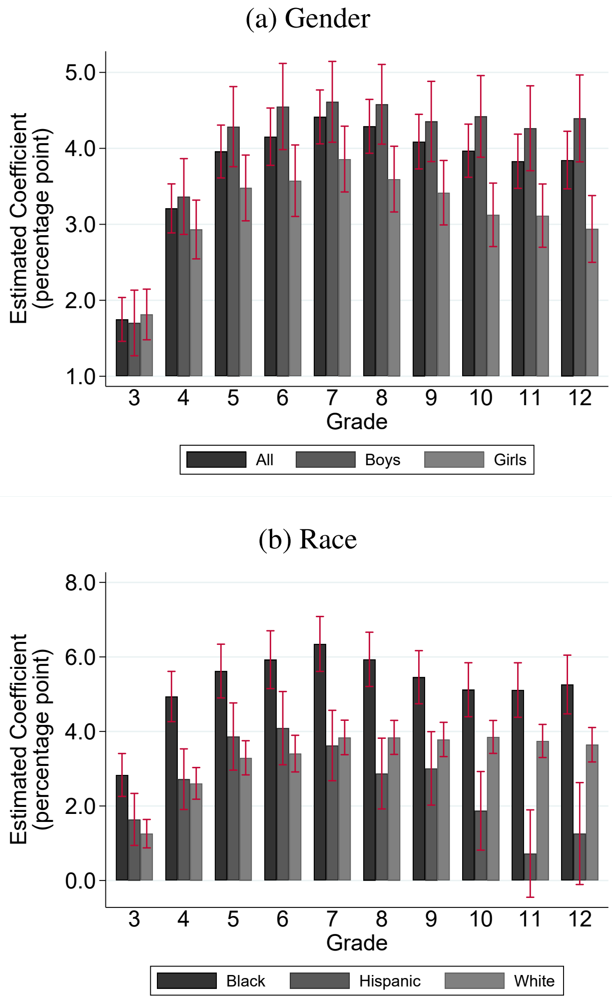
I begin by assessing the robustness of estimates to different bandwidths. I focus the bandwidth robustness exercise on my preferred specification, the local linear regression.[6] Figure 1.11 shows the estimated coefficient and 95% confidence interval for special education placement in grade 3 and end-of-grade 3 math and reading scores. The top figure indicates that for any bandwidth between 2 months and 5 months, the youngest children are more likely to receive special education services in grade 3. The estimated coefficient remains positive and ranges from 2.6 percentage points to 1.8 percentage points. These estimates are statistically significant at the 1 percent level. In panel (b) estimates for math and reading scores remain consistent across bandwidths from 2 months to 5 months. Figure 1.12 shows that estimates for high school exam scores are similarly robust to bandwidth choice; for any bandwidth between 2 months and 5 months, I find that children born the month before the cutoff date perform between one-tenth and one-fifth of a standard deviation lower on their end-of-course exams.

Next, I show that my estimates are robust to assumptions about the correlation of standard errors. Table 1.7 presents estimates of δ_1 from equation 1.2 for three specifications. The first specification two-way clusters at the school and relative month, the second specification reports heteroskedastic robust standard errors that are not clustered, and the final specification clusters standard errors at the school (the preferred specification used throughout this project). As can be seen, changing the level at which standard errors are clustered does not affect the statistical significance of the estimates. Standard errors are similar across all three specifications.

My main sample includes some individuals who leave the North Carolina public school

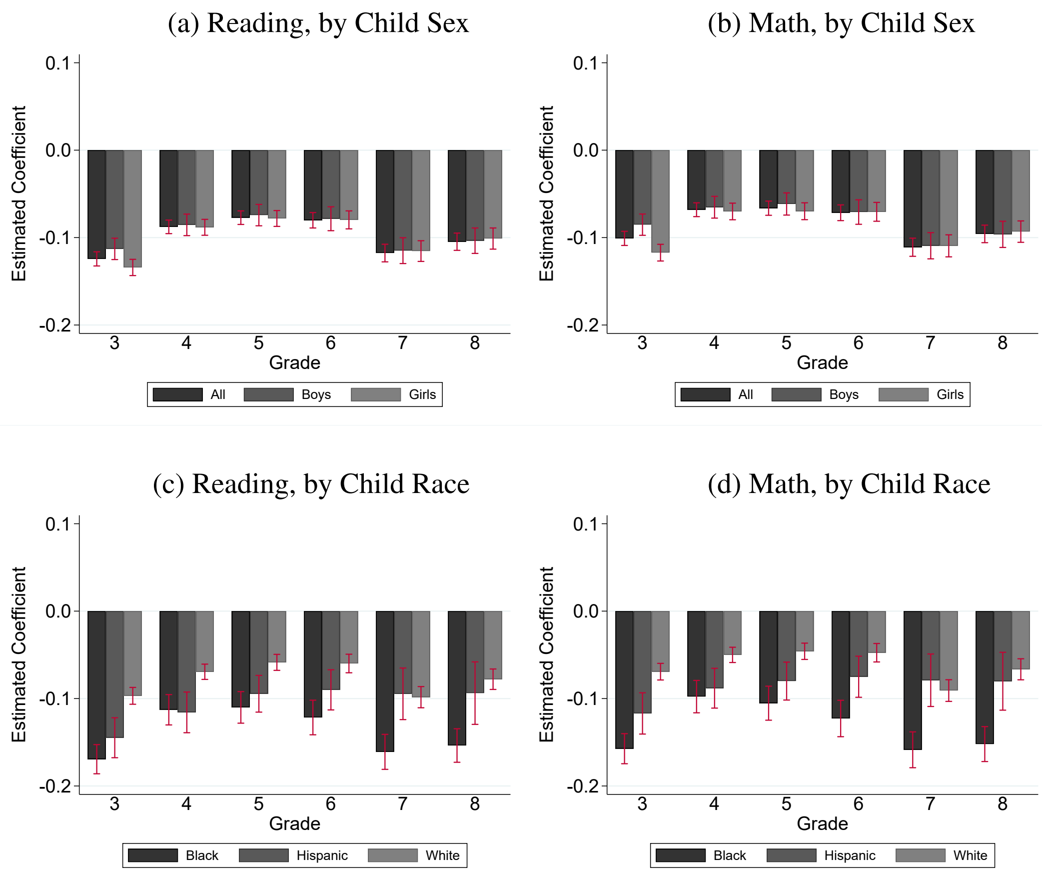
[6] I show that estimates are robust to inclusion of higher order polynomials, results from those specifications are shown in table A.1.

Figure 1.9: Heterogeneity of RD Estimates of the Effect of Relative Age on Special Education, by Gender and Race



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. Special education includes thirteen separate classifications of disabilities. These figures show the coefficient and 95 percent confidence interval for special education by type of diagnosis at a given grade by race and gender. The local linear regressions are estimated using robust standard errors clustered at the school with a bandwidth of 6 months. The effect of the school entry cutoff date persists through 12th grade for overall special education for all subgroups, except Hispanics.

Figure 1.10: End-of-Grade Reading and Math Z-Scores, by Gender and Race



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. These figures show the coefficient and 95 percent confidence interval for math and reading end-of-grade exams at a given grade by race and gender. The local linear regressions are estimated using robust standard errors clustered at the school with a bandwidth of 6 months. The effect of the school entry cutoff date persists through 8th grade for all subgroups.

Table 1.7: Regression Discontinuity Estimates of Being Youngest in Classroom on Grade 3 Outcomes, Clusters

	Age at Grade 3 Exam (1)	Grade 3 Math Z-Score (2)	Grade 3 Read Z-Score (3)	Receives SpEd in Grade 3 (4)	Repeated Grade 3 (5)
School, Relative Month					
Youngest	-0.5952***	-0.1009***	-0.1244***	0.0175***	0.0116***
	(0.0024)	(0.0037)	(0.0037)	(0.0014)	(0.0006)
No Cluster					
Youngest	-0.5952***	-0.1009***	-0.1244***	0.0175***	0.0116***
	(0.0018)	(0.0038)	(0.0038)	(0.0014)	(0.0006)
School (main specification)					
Youngest	-0.5952***	-0.1009***	-0.1244***	0.0175***	0.0116***
	(0.0030)	(0.0041)	(0.0041)	(0.0015)	(0.0006)
Dep. Mean	9.4511	0.0596	0.0638	0.1062	0.0158
N	1,083,943	1,075,424	1,074,108	1,083,946	1,083,946

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are reported in parentheses. Special education includes thirteen separate classifications of disabilities. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. Repeated grade is an indicator that the child was recorded in third grade in two consecutive years. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

system prior to high school graduation. However, children born the month before the cutoff date are 1.7 percentage points (8%) more likely to leave the NC public school system relative to their older classmates, and this estimate is statistically significant at the 1 percent level. Thus, in Table 1.8 and Table 1.9 I report estimates of equation (1.2) for the sample of individuals who remain in NC public schools through the last year of the data or high school graduation. I find that inclusion of individuals who leave the public school system prior to graduation are not driving my results.

Table 1.8: Regression Discontinuity Estimates of Being Youngest in Classroom on Grade 3 Outcomes, Non-Leavers

	Age at Grade 3 Exam (1)	Math Z-Score (2)	Reading Z-Score (3)	Receives SpEd (4)	Repeated Grade (5)
Youngest	-0.6250***	-0.0912***	-0.1147***	0.0186***	0.0106***
	(0.0033)	(0.0044)	(0.0043)	(0.0016)	(0.0007)
Dep. Mean	9.4405	0.0785	0.0818	0.1041	0.0132
N	898,385	893,099	892,359	898,386	898,386

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3 and remained in the sample through 2014. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Special education includes thirteen separate classifications of disabilities. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. Repeated grade is an indicator that the child was recorded in third grade in two consecutive years. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

For the sample of children who remain in NC public schools, I find that the children born the month before the cutoff date are 0.625 years younger at the time of their third grade exam, perform one-tenth of a standard deviation lower on math and reading end of grade 3 exams, are 1.86 percentage points (17.8%) more likely to be placed in special education, and are 1 percentage point (80%) more likely to be retained in grade 3 relative to their peers born the month after the cutoff date. Further, I find that the children born the month before the cutoff date perform approximately one-fifth of a standard deviation lower on end-of-course exams in Algebra, English, and Biology. Taken together, my estimates are remarkably robust to choice of bandwidth, the level at which standard errors are clustered, and to restricting the sample

Table 1.9: Regression Discontinuity Estimates of Being Youngest in Classroom on High School Tests, non-leavers

	Algebra		English		Biology	
	Z-Score	Grade Taken	Z-Score	Grade Taken	Z-Score	Grade Taken
	(1)	(2)	(3)	(4)	(5)	(6)
Youngest	-0.1722***	0.4726***	-0.2730***	0.5331***	-0.1839***	0.4966***
	(0.0086)	(0.0078)	(0.0087)	(0.0062)	(0.0106)	(0.0064)
Dep. Mean	0.0800	10.2871	0.1212	10.4159	0.0691	10.8616
N	267,592	267,592	255,009	255,009	189,354	189,354

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school who attended any grade between 9 and 12 and remained until graduation or until 2014. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. Grade taken is a grade level between 9 and 12 in which the child completed their End-of-Course exam. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

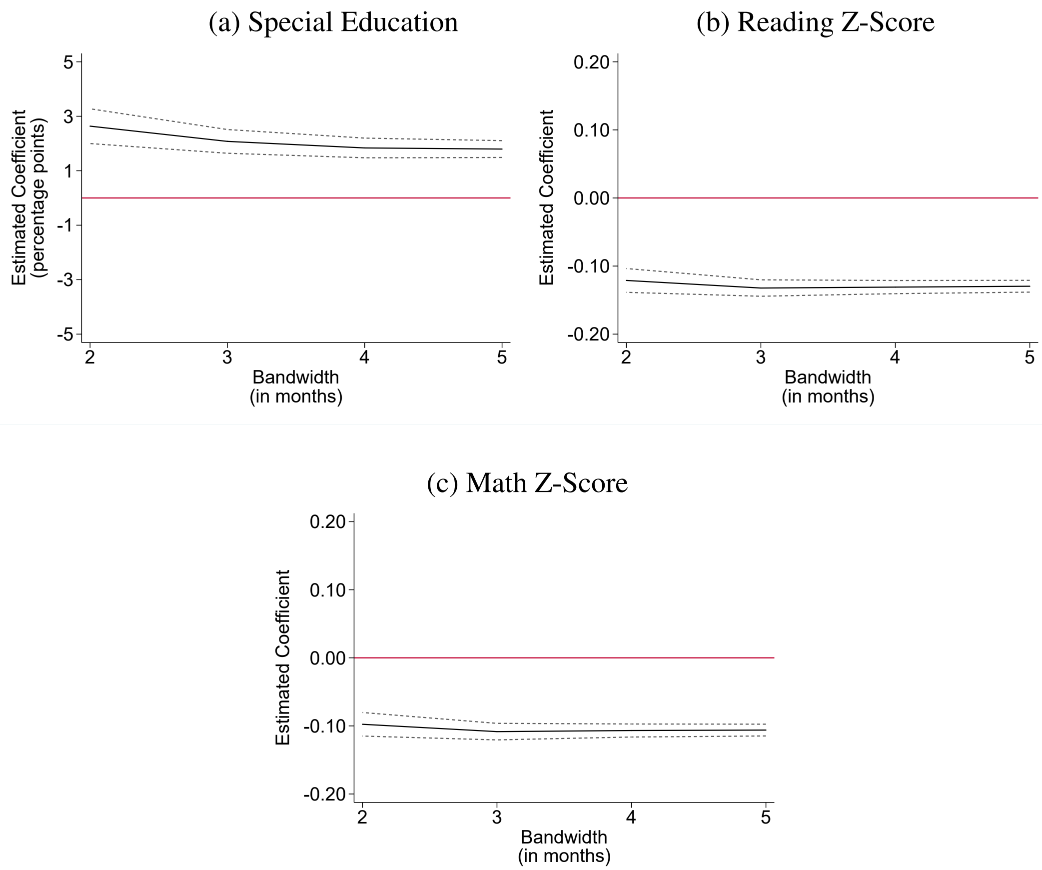
to individuals who remain in NC public schools. In other words, it does not appear that the association between month of birth and special education placement, grade retention, or test scores is spurious.

1.6 Conclusion

Recent work has shown that children born just before a school entry cutoff date are more likely to be diagnosed with ADHD compared to their peers born just after the cutoff date (Layton et al., 2018, Elder, 2010; Elder and Lubotsky, 2009). This higher prevalence of ADHD diagnosis for relatively young children is troubling if these children are incorrectly diagnosed due to their relative immaturity rather than an underlying biological disorder. These children would typically be diagnosed by their teacher and receive special education services with their school.

In this project, I evaluate the short- and long-term impact of school entry cutoff dates on a child's special education placement and tenure. I use administrative student level records from the state of North Carolina and regression discontinuity methods. Exogenous variation stems from a child's date of birth relative to the school entry cutoff date. I find that the youngest children in the classroom are more likely to receive special education services relative to their older

Figure 1.11: Robustness of RD Estimates to Bandwidth, Grade 3 Outcomes

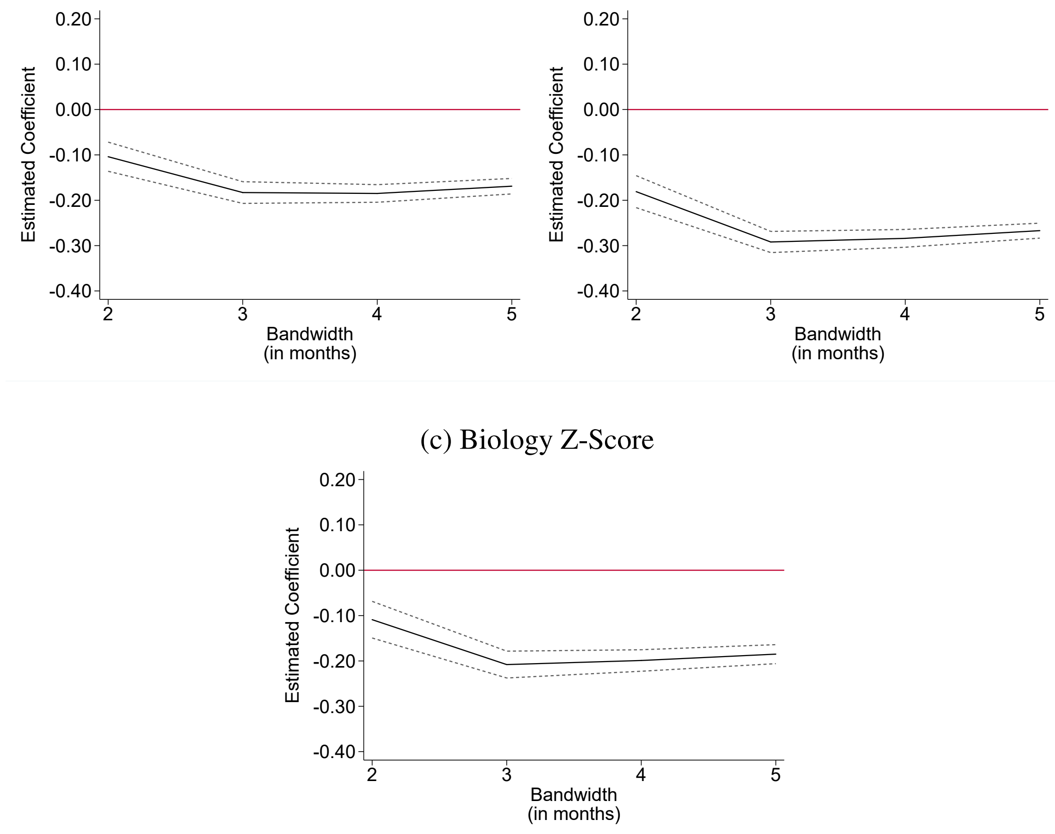


Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 3 through 12. Special education includes thirteen separate classifications of disabilities. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. These figures show the coefficient and 95 percent confidence interval for special education and EOG exams in math and reading by bandwidth. The local linear regressions are estimated using robust standard errors clustered at the school. The effect of the school entry cutoff date is robust to different bandwidths.

Figure 1.12: Robustness of RD Estimates to Bandwidth, High School Outcomes

(a) Algebra Z-Score (b) English Z-Score

(c) Biology Z-Score



Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for any number of grades in grades 9 through 12. Each test has been normalized within year, grade, and test subject to have a mean zero and standard deviation of one. These figures show the coefficient and 95 percent confidence interval for EOC exams in Algebra I, English II, and Biology by bandwidth. The local linear regressions are estimated using robust standard errors clustered at the school. The effect of the school entry cutoff date is robust to different bandwidths.

peers. Children born the month before the school cutoff date are 1.75 percentage points (16%) more likely to receive special education in grade 3 relative to their peers born the month after the cutoff date. This increased likelihood of special education is entirely driven by increases in the likelihood of being diagnosed with a subjective impairment. Importantly, I find no discontinuity in the likelihood that children born the month before the cutoff date are diagnosed with a visual/hearing impairment or autism.

I corroborate early work showing that the relatively youngest children perform worse on end-of-grade exams. In particular, children born the month before the cutoff date score approximately one-tenth of a standard deviation below their peers born the month after the cutoff date on both math and reading exams. This finding is smaller in magnitude than that found by Dhuey et al. (2017). This discrepancy may be caused by data limitations discussed in the text. Additionally, I find that children born the month before the cutoff date are 2.3 percentage points (28%) more likely to be retained in any grade between grades 3 and 12. Contrary to previous work, I find that the gaps in special education placement and test scores among children born around school entry cutoff dates persist through high school and are similar in magnitude across gender.

My results suggest that a negative feedback loop, in which the youngest children are placed on a lower track at the onset of their school exists. Importantly, my estimates only identify that a gap exists between the relatively oldest and relatively youngest within the classroom. This discontinuity could be made up of over-diagnoses for the relatively youngest, under-diagnoses for the relatively oldest, or more likely a combination of both. Overall, my results suggest that parents, teachers, and policymakers should be more cognizant of relative maturity within the classroom when determining a child's need for special education.

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CHAPTER 2

IS SPECIAL EDUCATION A PATHWAY TO

SUPPLEMENTAL SECURITY INCOME FOR CHILDREN?

2.1 Introduction

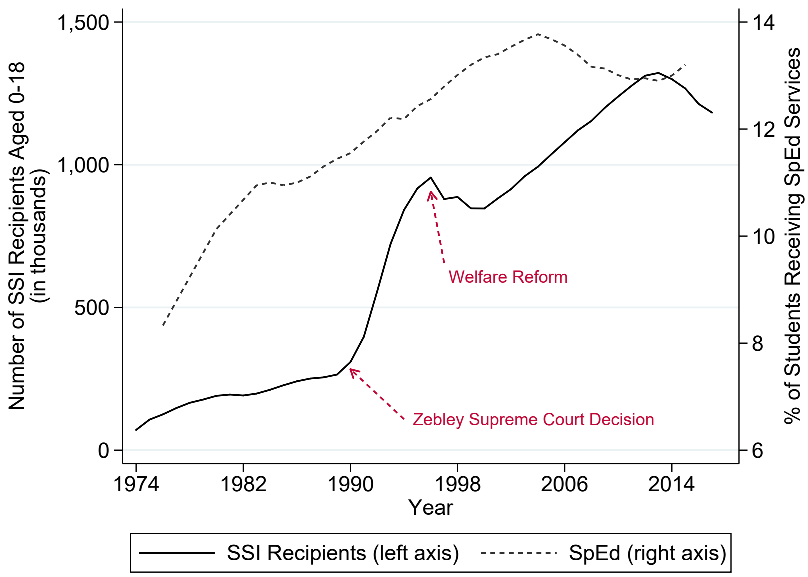
In 2017, the Social Security Administration paid $9.6 billion to approximately 1.2 million disabled children who receive Supplemental Security Income (SSI). The majority of these child recipients have been diagnosed with a mental impairment including: attention deficit hyperactivity disorder (ADHD), learning disability, and speech/language delays. These types of impairments are often established by a child's local school. I document a causal channel from receipt of special education services to application for and award of SSI. Specifically, I test whether children who are exogenously induced to have a higher predisposition of receiving special education services have a higher likelihood of applying for and receiving disability payments. I make use of the fact that otherwise identical children born around a kindergarten entrance eligibility cutoff date enter school at different relative-ages, and thus have a differential likelihood of receiving special education services (Elder and Lubotsky 2009, Dhuey and Lipscomb, 2010).

Several explanations for growth in the child SSI program have been posited. One hypothesis for the persistent growth of the child SSI caseload is an increase in the fraction of children receiving special education services (Aizer, Gordon and Kearney, 2013). This hypothesis stems from two main facts: first, growth in SSI has been attributed to an increase in the number of children diagnosed with mental impairments (Aizer, Gordon and Kearney, 2013), and presumably these children with mental impairments are receiving services with their local school (Duggan, Kearney and Rennane, 2015).[1] Second, a 1990 Supreme Court decision (*Sullivan v. Zebley*) liberalized the standard to evaluate child SSI applicants based on the child's ability to behave in an age-appropriate manner. This often meant that SSI judges were able to consider teacher and school counselor reports. By 2012, approximately 63% of child SSI determinations involved the use of teacher assessments (Government Accountability Office). Graphically, special education and SSI show similar trends; figure 2.1 shows the number of child recipients and the fraction of

[1] Currently, non-physical disabilities account for nearly 70% of all child SSI cases (Duggan, Kearney and Rennane, 2015; Aizer, Gordon and Kearney, 2013)

children receiving special education services between 1976 and 2015. Between 1976 and 2013 the fraction of children receiving special education services grew from 8.9% to 13%, while the child SSI roll increased from 0.26 million in 1989 to 1.3 million in 2013.

Figure 2.1: SSI Recipients and Fraction of Children Receiving Special Education Services 1976-2015



Data Source: Supplemental Security Annual Statistical Supplement 1998-2017 and National Center for Education Statistics Digest of Education Statistics 1995-2017.

To estimate the direct effect of special education on SSI, I implement a two-sample fuzzy Regression Discontinuity (RD). The first-stage sample utilizes information on all children eligible for kindergarten cohorts 2006, 2007, and 2008 enrolled in public schools in North Carolina from fall 2004 through fall 2014. Using a RD design, I estimate that children born the month before the school cutoff date are 3.3 percentage points more likely to be receive special education services at any time between the ages of 5 and 12 relative to their classmates born the month after the school cutoff date.

Next, I document a discontinuous increase in the likelihood a child applies for and is awarded SSI for children born just before the school cutoff date relative to children born just after the cutoff date. I use restricted versions of the National Health Interview Survey (NHIS) linked to Social Security Administration records. The data comprise a nationally representative sample of households in the US between 1994 and 2005. I show that children born just before the school cutoff date are 0.78 percentage points more likely to apply for SSI relative to children born just after the cutoff date. Further, children born directly before the cutoff date are 0.55 percentage points more likely to have an award for SSI relative to children born directly after the cutoff date. Estimates are statistically significant at the 1% level, robust to a wide variety of specification choices, and substantially greater than at nearby placebo cutoff dates. Importantly, using earlier birth cohorts, I do not find any increase in SSI applications and awards at the school cutoff date when it was not possible to use special education as documentation of child disability.

I obtain two-sample fuzzy RD estimates by combining the first-stage and reduced-form estimates. I find that a 1 percentage point increase in the fraction of children receiving special education services induces a 0.23 percentage point increase in the fraction of children applying for SSI and a 0.16 percentage point increase in the fraction of the children with an award for SSI. Estimates are statistically significant at the 1% level. Since 1990, the fraction of children receiving special education services increased 1.5 percentage points. Thus, my estimates suggest that the growth in special education allowed approximately 175,000 more children access to SSI payments. Back of the envelope calculations suggest that approximately 18% of the growth in the child SSI caseload since 1990 can be attributed to rising special education rates and spillovers between these two programs.

I interpret these increases in child SSI applications and awards for children born just before the school cutoff date as intent-to-treat (ITT) effects of early eligibility to school entry for those on the margin of being referred for special education services. My approach follows other studies using school entry eligibility cutoff dates to understand the intent-to-treat impact of relative-age-effects on child outcomes (e.g. Black, Devereux and Salvanes, 2011; Bedard and Dhuey, 2006; Dobkin and Ferreira, 2010; McCrary and Royer, 2011, Dhuey, Figlio, Karbownik and Roth, 2017).

By disaggregating awards into mental and physical awards, I show that the increase in child SSI awards for children born just before the school cutoff date are entirely driven by increases in awards for mental impairments. Children born directly before the school cutoff date are 0.52 percentage points more likely to receive an award for a mental impairment relative to children born directly after the cutoff date. I find no discontinuity in the likelihood a child is awarded SSI for a physical impairment for children born around the cutoff date. This makes intuitive sense;

unlike emotional and behavioral disorders, physical impairments can not be based on subjective assessment of a child in relation to his peers.

In analyses disaggregated by child sex, I find that the point estimates are larger for boys than for girls. Boys born directly before the school cutoff date are 1.08 percentage points more likely to apply for SSI and 0.77 percentage points more likely to receive an award for SSI relative to boys born directly after the cutoff date. On the other hand, girls born directly before the cutoff date are 0.45 percentage points more likely to apply for SSI and 0.31 percentage points more likely to have an award for SSI relative to girls born directly after the cutoff date. These heterogeneous effects by child sex are consistent with the fact that boys account for two-thirds of child SSI awards, make up the majority of special education placements, and tend to be more responsive to adverse shocks than girls at earlier ages.[2]

I contribute to the literature on child disability in three ways. First, I causally identify an interaction between special education and SSI. In doing so, I augment the sparse literature on the interaction between special education and SSI, which I discuss below. Second, my estimates indicate that a school district's decisions on identification and classification of children for special education services affects the federal SSI roll. This has policy implications for both local schools identifying children in need of special education services as well as the Social Security Administration (SSA) as it attempts to adequately address child disability needs. Third, I show that an arbitrary date affects a child's likelihood of applying for and receiving SSI. To the best of my knowledge, these are the first causally identified estimates of the direct effect of receipt of special education services on SSI.

There are several reasons special education may interact with SSI. First, special education and SSI both require a child to exhibit a mental or physical disability which inhibits the ability

[2]For instance, Autor, Figlio, Karbownik, Roth and Wasserman (2016a) show that relative to their sisters, boys born to disadvantaged families have higher rates of disciplinary problems, lower achievement scores, and fewer high-school completions. Similarly, Autor, Figlio, Karbownik, Roth and Wasserman (2016b) show that cumulative exposure to higher quality schools benefits boys more than their sisters. Bertrand and Pan (2013) show that non-cognitive development differs by gender; specifically boys' non-cognitive development is extremely responsive to worse parental inputs while girls' non-cognitive development is only weakly influenced by parental inputs. Campbell, Conti, Heckman, Moon, Pinto, Pungello and Pan (2014) and Conti, Heckma and Pinto (2015) find that intensive early educational programs differentially improved boys' long term health. Chetty and Hendren (2018) find that growing up in a low-mobility neighborhood has a larger adverse effect on a boy's economic mobility than a girl's economic mobility. Finally, Fan, Fang and Markussen (2015) find that maternal employment during early childhood reduces boys' eventual educational attainment relative to that of girls'. Therefore, my estimates document an additional area where boys are more responsive to a policy shock.

of the child to perform in a manner appropriate for his age; this often means that children are assessed based on school performance including test scores and classroom behavior (Cohen, 2007). Second, schools increasingly provide services beyond instruction including: informing families about medicaid eligibility, providing children with school based health services, and providing children with counseling services. In addition to providing children with resources to aid and enhance their education, schools may provide parents with information concerning their eligibility for SSI. Third, special education may reduce the cost of applying for SSI by providing a family with documentation of a child's impairment. Applying for SSI benefits is costly. Parents must have clear documentation of the child's disability and how the disability restricts the child's ability to function in an age appropriate manner. Submitting this documentation to an SSA judge can create a financial burden. Once SSA allowed school counselor reports as documentation of a child's disability, that is after the 1900 *Zebley* decision, the cost for an SSI application was significantly reduced if the child was receiving special education services. This reduction in cost occurs because screenings for special education services are free to parents whose children are in public schools.[3] All three mechanisms may exist simultaneously. Available data do not allow me to disentangle each mechanism separately.

Despite the potential connections between SSI and special education, little work has been able to identify the effect of special education on SSI, nor the inverse, that of SSI on special education. This is due to the difficulty of disentangling the causal relationship running from SSI to special education from the causal pathway running from special education to SSI. Two exceptions are Cohen (2007) and Cullen and Schmidt (2011) which both find a positive relationship between special education and SSI. Cohen (2007) explores the effect of SSI on special education. Utilizing variation generated by the difference between the AFDC benefit schedule and the SSI benefit a child would be eligible to receive, Cohen finds that after the 1990 *Zebley* decision, states with less generous AFDC schedules had larger increases in special education enrollments. However, local economic shocks may have endogenously affected both a state's AFDC benefit schedule and a child's propensity to be screened for special education services. My work complements that of Cohen, showing that special education and SSI simultaneously

[3]Under the Individuals with Disabilities Education Act of 1975 and the Disabilities Education Improvement Act of 2004, schools have an obligation to identify, locate, and evaluate students who they suspect may have a disability. Schools must also provide students identified with a disability a free and appropriate education (FAPE).

interact.

Similar to this study, Cullen and Schmidt (2011) study the relationship from special education to SSI. They implement a ordinary least squares estimation strategy to show that counties faced with a school finance incentive to classify a greater number of students for special education services also experienced larger gains in their SSI caseload. However, they were not able to rule out that other differences within counties contributed to the differential growth in SSI caseload. The benefit of this study is that I can directly identify a spillover using exogenous variation in the likelihood a child receives special education services.

More recently, a descriptive report by Aizer, Gordon and Kearney (2013) finds a positive correlation between the special education rate and the SSI award rate within a state, suggesting a possible link between special education participation within the state and child SSI caseload. Because they find no association between the special education rate and the SSI application rate, they hypothesize that special education may simply increase the likelihood that a child is awarded SSI because special education provides documentation of the child's impairment. Contrary to this hypothesis, my estimates suggest that receipt of special education does induce an increase in the likelihood a child applies for SSI (at least among compliers).

My estimates apply to children born directly around the school cutoff date, who may differ from children born in months further away from the school cutoff date. For example, Buckles and Hungerman (2013) document that children born in December and January are more likely to have teenage mother, more likely to have an unmarried mother, and more likely to have a mother with only a high school degree. However, since I estimate effects using exact date of birth rather than season of birth, my estimates are not contaminated by this seasonal variation. I confirm this using tests for differences in observable child characteristics for children born around the cutoff date. Further, Dickert-Conlin and Elder (2010) show that parental characteristics are smooth through school cutoff dates; suggesting that there is no evidence that parents time births around school cutoff dates.

This paper proceeds as follows. Section 2.2 provides background information about SSI for children and school cutoff dates. Section 2.3 discusses the data and sample. Section 2.4 presents the empirical strategy. Section 2.5 outlines the change in child disability for children born around the cutoff date. And Section 2.8 concludes with a discussion.

2.2 Institutional Framework

2.2.1 Supplemental Security Income for Children

SSI is a federal means-tested program under Title XVI of the Social Security Act. To qualify for child SSI the child must be under 18 and have a physical or mental condition that results in "marked and severe functional limitations." Since SSI is a means-tested program, SSI eligibility and payment amounts depend on income and assets. In 2018 the federal benefit rate (FBR), or maximum monthly benefit level was $750 for an eligible individual. Over 60% of SSI children receive the maximum benefit (Coe and Rutledge, 2013), and in June 2018, the average monthly payment for children was $662 (Social Security Administration, 2018a).

To determine a child's income eligibility, the Social Security Administration (SSA) looks at a child's earnings plus a fraction of the income and assets of family members which have been "deemed" to the child. The deeming rules are such that in 2018, an SSI eligible child in a one-parent household could qualify for the maximum SSI payment if the family earned as much $1,585 a month and there were no other children in the household or $1,960 if there was one non-disabled sibling in the household.[4] In 2016, only 16 percent of children have any income deemed to them (Social Security Administration, 2017). Further, of SSI children living with a single parent, 46% reported no parental income and those with reported incomes had average monthly incomes of $1,477 (Social Security Administration, 2017).

Despite the generosity of the SSI program, take-up is incomplete (Currie, 2004). While there are no estimates for the participation rate of eligible children, take-up rates of the eligible elderly hover around 55%. Two main barriers to participation cited in the literature are, one, a lack of information about program eligibility (Coe, 1985), and two, the cost to apply is prohibitive (Bound, Kossoudji and Ricart-Moes, 1998). Interactions among local, state, and federal agencies may reduce these barriers to SSI participation. For example, SSI often provides automatic enrollment in Medicaid, SSI recipients have particularly high participation in SNAP, and evidence suggests fiscal spillovers between AFDC and SSI. Most recent work on child SSI recipients has focused on the relationship between SSI and AFDC/TANF. Financial incentives induced both states and families to switch beneficiaries from AFDC to SSI. Kubik (1999) and Garrett and

[4] Income levels were calculated using deeming eligibility for children and information provided by SSA. Available at: https://www.ssa.gov/ssi/text-income-ussi.htm

Glied (2000) show that SSI take-up after the 1990 *Zebley* decision is related to the financial gain a family would garner from switching from AFDC to SSI. Further, Kubik (2003) finds that states with higher fiscal deficits induced a greater number of AFDC recipients to switch to the SSI roll. This is because AFDC benefits require the state to provide a match, while SSI benefits do not.

Descriptive works by Duggan, Kearney and Rennane (2015) and Aizer, Gordon and Kearney (2013) note that child SSI program should interact with local education systems for establishing disabilities. However, this interaction has been difficult to causally identify. Prior to 1990 a child could be found disabled if his impairment met the criteria of a listed medical condition in the program's "Listing of Impairments." An adult, however, could be found disabled if an assessment of residual functional capacity showed that he could not engage in work in the national economy. The February 1990 *Sullivan v. Zebley* case centered on the Supreme Court's ruling that SSA's regulations for determining disability in children were in violation of the SSI statute, because there was no functional assessment to determine if a child was disabled.[5] The law afforded that a child would be found disabled if he or she had a medically determinable physical or mental impairment of comparable severity to one that would disable an adult. Thus, new regulations imposed that SSA conduct Individualized Functional Assessments (IFA) to determine if a child's impairment prohibited a child from functioning day-to-day in age-appropriate behaviors at home, at school, and in their communities (Coe and Rutledge, 2013). Additionally, in December 1990, SSA expanded the list of childhood mental impairments. These changes to child SSI eligibility in 1990 enabled SSI judges to consider school counselor and teacher evaluations when determining child disability. Thus, the 1990 *Zebley* decision opened up a direct channel for local agencies, schools, to provide information about eligibility to the family and significantly reduced the cost of applying for SSI. This channel, addresses the two largest barriers to program participation identified by (Currie, 2004), and should therefore increase the participation rate of eligible children. In 2012, over half of all child SSI determinations utilize school and teacher evaluations (Government Accountability Office, 2012).

Since 1990, the number of child applicants has quadrupled, and the allowance rate increased from 30% to 50% (Coe and Rutledge, 2013). Most of the growth came from children suffering from mental impairments including ADHD, learning disabilities, and behavioral problems

[5] See Coe and Rutledge (2013) for a detailed overview of the *Zebley* changes.

(Duggan, Kearney and Rennane, 2015). Currently, non-physical disabilities account for nearly 70% of all child SSI cases (Duggan, Kearney and Rennane, 2015; Aizer, Gordon and Kearney, 2013).[6]

2.2.2 School Entry Cutoff Dates and Special Education

Under the Individual's with Disabilities Act of 1975, districts are mandated to provide all students with a free and appropriate education (FAPE). While IDEA provides general definitions of disability, local state agencies create their own precise diagnostic guidelines. Schools are charged with formally identifying children with disabilities and providing services (Cullen and Schmidt, 2011). Recent research has identified that teacher identification of disability can be idiosyncratic--driven by fiscal incentives (Cullen, 2003), pressures by school accountability systems (Prenovitz, 2018), and subjective assessment of child maturity (Elder and Lubotsky, 2009; Dhuey and Lipscomb, 2010).

Elder and Lubotsky (2009) show that there are two negative effects of being the youngest child in the classroom. First, having older peers increases the likelihood of repeating kindergarten, first, or second grade by 13.1 percentage points. Second, having older peers increases the likelihood that a child is diagnosed with ADHD before 3rd grade by 2.9 percentage points. Given the base diagnosis rate of 4.3 percent in their sample, these estimates indicate that children born just before a school entry cutoff date are 67% more likely to be diagnosed for ADHD than their older classmates who were born just after the cutoff date. Further, children born before the cutoff date are more likely to use prescription medications, such as Ritalin, to treat ADHD (Elder, 2010). The higher prevalence of ADHD diagnosis and medication for relatively young children is troubling if these children are incorrectly diagnosed due to their relative immaturity rather than an underlying biological disorder. Elder (2010) notes that the true prevalence of ADHD remains unknown, thus his estimates could indicate that younger children may be over-diagnosed, older children may be under-diagnosed, or more likely a combination of both.

[6] It should be noted that the passage of the Personal Responsibility and Work Opportunity Act (Welfare Reform) in 1996 eliminated the "functional assessment" for children, as well as increased the likelihood that a child was removed from SSI during a continuity disability review (CDR). As a result of Welfare Reform, the eligiblity criteria for children became more stringent after 1996. As one would expect, I find the largest effect sizes for children eligible for school entry between 1990 and 1996. Results disaggregated by birth cohort are imprecisely estimated and are not presented.

Dhuey and Lipscomb (2010) extend this work, finding that the effects of youth-in-grade on disability persist through 10th grade. Using three national surveys they find that each additional month of age relative to the cutoff date is associated with a 2 to 5 percent reduction in the likelihood of ever receiving special education services for learning disabilities. They further show that prior to grade 3, the effect of relative-age-in-grade primarily affects boys, where they find no effect of relative-youth-in-grade for girls prior to grade 3.

2.2.3 Statewide School Entry Cutoff Dates

Like the prior work of Elder and Lubotsky (2009) and Dhuey and Lipscomb (2010), my research design rests on institutional policies within each state that determine the age at which a child is eligible to enter kindergarten. The majority of states require a child to turn 5 on or before a statewide cutoff date for entry to kindergarten. Most states set this cutoff date in late August or early September of the calendar year in which the child turns five. For instance, Minnesota requires students to turn 5 on or before September 1 of the school year they enter kindergarten. Thus, the kindergarten class of 2000 was made up of children born between September 2, 1994 and September 1, 1995.

Consider a child born on September 1, 1995 and a child born September 2, 1994. These two students are classmates but are essentially one year apart in biological age when they enter school. The child born on September 1, 1995 is 5 when entering kindergarten whereas the child born on September 2, 1994 is nearly 6 when entering kindergarten. States vary their entry cutoff dates and these dates have changed over time.[7] My research leverages the variation in the relative-age of each child in their grade based on these school entry cutoff dates. I utilized the school entry cutoff dates for school years 1964-2005 as documented by Bedard and Dhuey (2006). All dates were confirmed using state legislation statutes. Table 2.1 shows the statewide cutoff dates for select cohorts. The main sample includes states that have a statewide cutoff date for the entire sample period between 1964 and 2007.

[7]For example, between 1992 and 1996, Delaware phased in a school cutoff date moving the cutoff date from December 31 to August 31.

Table 2.1: State School Entry Eligibility Cutoff Dates for Select Years 1964-2000

State	Legislation	SY 1964 1957 Cohort	SY 1983 1978 Cohort	SY 2000 1995 Cohort	State	Legislation	SY 1964 1957 Cohort	SY 1983 1978 Cohort	SY 2000 1995 Cohort
Alabama	AL Code §16-28-4(b)	1-Oct	1-Oct	1-Sep	Montana	MT Code §20-7-117	SSY	10-Sep	10-Sep
Alaska	AK Stat §14.03.080(d)	2-Nov	2-Nov	15-Aug	Nebraska	NE Rev Stat §79-214	15-Oct	15-Oct	15-Oct
Arizona	AZ Rev Stat §15-821(c)	1-Jan	1-Sep	1-Sep	Nevada	NV Rev Stat §392.040	31-Dec	30-Sep	30-Sep
Arkansas	AR Code §6-18-207(a)	1-Oct	1-Oct	1-Oct	New Hampshire	Not specified in statute	30-Sep	30-Sep	30-Sep
California	CA Educ Code §48000(a)	1-Dec	1-Dec	2-Dec	New Jersey	NJ Rev Stat §18A:44-2	LEA	LEA	LEA
Colorado	CO Rev Stat §22-1-115	LEA	LEA	LEA	New Mexico	NM Stat §22-13-3 (d)	1-Jan	1-Sep	1-Sep
Connecticut	CT General Stat Sec §10-15c(a)	1-Jan	1-Jan	1-Jan	New York	NY Educ L §1712	1-Dec	1-Dec	1-Dec
Delaware	DE Code §14-27-02	1-Sep	31-Dec	31-Aug	North Carolina	NC Gen Stat §115C-364	1-Oct	16-Oct	16-Oct
Florida	FL Stat §1003.21	1-Feb	1-Feb	1-Sep	North Dakota	ND Cent Code §15.1-06-01	31-Oct	31-Aug	1-Sep
Georgia	GA Code §20-2-150	none	none	1-Sep	Ohio	OH Rev Code §3321.01	none	30-Sep	30-Sep
Hawaii	HI Stat §302A-411	31-Dec	31-Dec	31-Dec	Oklahoma	OK Stat §70-18-108	1-Nov	2-Nov	1-Sep
Idaho	ID Code §33-201	16-Oct	16-Oct	1-Sep	Oregon	ORS §336.092	1-Nov	1-Sep	1-Sep
Illinois	IL Compiled Stat §105-5-26	1-Dec	1-Dec	1-Sep	Pennsylvania				LEA
Indiana	IN Code §20-33-2-7	none	none	1-Jun	Rhode Island	RI Gen Laws §16-2-27	none	31-Dec	31-Dec
Iowa	IA Code §282.3 (b)	15-Oct	15-Sep	15-Sep	South Carolina	SC Code §59-63-20	none	1-Nov	1-Sep
Kansas	KS Stat §72-1107(c)	1-Sep	1-Sep	31-Aug	South Dakota	SD Code §13-28-2	1-Nov	1-Sep	1-Sep
Kentucky	KY Stat §158.030	31-Dec	1-Oct	1-Oct	Tennessee	TN Code §49-6-201	31-Dec	31-Oct	30-Sep
Louisiana	LA Rev Stat §17:222(a)	31-Dec	31-Dec	Sep-31	Texas	TX Educ Code §29.151	1-Sep	1-Sep	1-Sep
Maine	ME Rev. Stat Title 20-A §5201	15-Oct	15-Oct	15-Oct	Utah	UT Code §53A-3-402(6)	SSY	SSY	2-Sep
Maryland	MD Reg 13A.08.01.02 (b)	31-Dec	31-Dec	31-Dec	Vermont	16 VSA §1073	1-Jan	1-Jan	LEA
Massachusetts	M.G.L. 603 CMR §8.02	LEA	LEA	LEA	Virginia	VA Code §22.1-199	30-Sep	30-Sep	30-Sep
Michigan	M.C.L §380.1147	1-Dec	1-Dec	1-Dec	Washington		SSY	LEA	LEA
Minnesota	MN Stat §124D.02	1-Sep	1-Sep	1-Sep	West Virginia	WV Code §18-5-18	none	1-Sep	1-Sep
Mississippi	MS Code §37-15-9	1-Jan	1-Sep	1-Sep	Wisconsin	WI Stat §118.14	1-Dec	1-Sep	1-Sep
Missouri	MO Rev Stat §160.053.1	1-Oct	1-Oct	15-Aug	Wyoming	WY Stat §21-4-302	15-Sep	15-Sep	15-Sep

Notes: School entry cutoff legislation dates were reported in Appendix 1 of Bedard and Dhuey (2006) for school years 1964-2005. These dates were corroborated using published reports by the Education Commission of the States in 2010, 2011, and 2014. Using specific legislative codes reported in the 2010 publication, I verified each state's cutoff date and documented more recent legislative changes through 2016. When dates conflicted among sources, I reported the date recorded by state statute.

2.3 Data

2.3.1 First-Stage Sample

North Carolina, like the majority of states, adheres to a minimum age of enrollment into public kindergarten through its compulsory schooling law. Prior to the 2009-2010 school year, children who achieved the age of five on or before October 16 were eligible to enter school in the fall of the calendar year in which they turned five.[8] I use individual student level data from the North Carolina Department of Public Instruction (NCDPI), provided by the North Carolina Education Research Data Center (NCERDC). The data covers all students in North Carolina's public school system with detailed information on children in grades 3 through 12. Since 2005, the data includes a summary file for children between the ages of 3 and 21 who receive special education services.

There are several limitations to this data: first, student date of birth is recorded as month of birth, rather than exact date of birth; second, I cannot follow students who chose to leave the public school system.[9] If the group of students leaving the public school system were systematically different from those initially enrolled, and this difference was driven by the relative age of the child, my estimates would be biased. I address both concerns. First, I drop all October births for cohorts facing the October 16th cutoff date. This is because October births represent both the oldest and youngest students in their grade. Second, to determine if differential attrition occurs in my sample, I look for differences in the likelihood a child leaves NC public schools prior to graduation for those born at the cutoff date. I limit my sample to those eligible for kindergarten cohorts 2006, 2007, and 2008 who remained enrolled in the NC public school system through fall 2014.[10]

[8] In 2009 the law was amended, pushing the date forward from October 16 to August 31, thereby increasing the average age of children within each cohort. There is no reason to expect the October 16 cutoff date to behave differently than any other school cutoff date. Within North Carolina, the August 31 cutoff date generates similar estimates for children eligible for kindergarten cohorts 2009-2012. Additionally, when I compare tests scores of children born around the cutoff date, the estimates from North Carolina in which children face the October 16 cutoff date, are remarkable similar to those found by Dhuey et al. (2017) for children enrolled in Florida public schools facing a September 1 cutoff date. Appendix Figures B.5 and B.6 indicate that North Carolina is similar among observables characteristics to the US in general.

[9] It should be noted that in the first-stage sample, I compare children born the month before the cutoff date to the children born the month after the cutoff date. Whereas in the reduced-stage sample, discussed below, I am comparing children born the day of the cutoff date to children born the day after the cutoff date. This is due to data limitations, but should not affect the interpretation of results.

[10] In a secondary first-stage sample, I estimate the effect of receiving special education services between kindergarten and grade 6 including the individuals who leave the NC public school system prior to grade 6. These estimates

Despite the limitations, this administrative data provides more finely detailed observations than previous research utilizing smaller national panel surveys. Further, this data improves upon survey data by providing administrative records of children receiving special education services. For instance, while the NHIS does ask if a child receives special education services, only 7 percent of children report receipt of services when the national average is 13%.[11] Administrative records allow for more accurate analysis of the relationship between school entry age and special education.

To construct the analysis sample, I combine the masterbuild file for years 2004-2014 with the exceptionality file.[12] Data is constructed such that each child appears as a single observation. I then create an indicator for whether the child received special education services between the ages of 5 and 12.[13] This variable is constructed to match the format of the outcomes in the reduced form sample discussed below. I restrict analysis to the kindergarten cohorts 2006 to 2008. These cohorts are the individuals for whom valid information exists for each year between kindergarten and grade 6.[14]

Table 2.2 displays descriptive statistics for North Carolina public school students. As can be seen, children born just before the cutoff date are approximately half of a year younger at the time of their third grade End-of-Year test. Thus, school entry cutoff date does appear to affect the age of children within the grade, as expected. In terms of covariate balance around the cutoff date, I find no differences in the fraction of children who are male, free and reduced price lunch eligible, or who leave the NC public school system early. Children born before the cutoff date are more likely to repeat a grade and to be referred to the English Language Program.[15]

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are available in appendix table B.10. Inclusion of students who leave the sample prior to 6th grade does not affect the main findings. Of the two samples, I present the one that provides the most conservation estimate for the effect of special education on SSI.

[11] Special education services are only asked if the individual is between the ages of 7 and 17 at the time of the NHIS survey. My sample includes individuals outside this age range at the time of the survey. In this paper I do not present any estimates of the increase in reported receipt of special education using the NHIS sample.

[12] School years are denoted by the fall calendar year, thus 2004 represents the 2004-2005 school year.

[13] North Carolina public school records contain the the grade a child was enrolled in during the year rather than their age, I use grades kindergarten through grade 6 to mean the child was aged 5 to 12.

[14] Results are similar if I use information for a greater number of kindergarten cohorts who do not have information on special education services prior to grade 3.

[15] Appendix Figure B.3 shows that characteristics of individuals evolve smoothly through the cutoff date. Appendix figures B.4, B.5,and B.6 speak to the generalizability of North Carolina. North Carolina tends to have a larger Hispanic population than the US average, and a smaller black population relative to the US average, but trends similarly to the US average in terms of characteristics of children.

Table 2.2: Descriptive Statistics For Children Born Within 6 Months of Statewide Cutoff Date, North Carolina

	All		Boys		Girls	
	Relative Age>0 Youngest	Relative Age <0 Oldest	Relative Age>0 Youngest	Relative Age <0 Oldest	Relative Age>0 Youngest	Relative Age <0 Oldest
Age-at-Test in Grade 3	9.08	9.45	9.13	9.46	9.04	9.43
Special Education (%)	12.4	11.4	15.6	14.5	9.3	8.5
Leave sample early	12.7	12.6	12.8	12.9	12.2	12.3
English LL	12.7	11.1	13	11.4	12.4	10.7
FRL Eligible	58.6	58.7	58	58.2	59.2	59.2
Ever Repeat Grade	4.1	3.3	4.7	3.9	3.4	2.7
Characteristics						
White	52.3	51.3	52.6	51.8	51.9	50.7
Black	24.2	25.6	23.8	25.1	24.6	26.1
Hispanic	15.3	14.7	15.5	14.9	15.2	14.6
Male	48.8	49.3				
N	133,254	126,533	65,741	61,725	67,540	64,808

Data Source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Includes children in kindergarten cohorts 2006, 2007, and 2008. All October births are dropped from the sample. English Language Learner (English LL) and Free and Reduced Price Lunch (FRL Eligible) indicate the child was enrolled for the program at any time between kindergarten and grade 6. Race categories are mutually exclusive, and other is the omitted category.

2.3.2 Reduced-Form Sample

I use National Health Interview Survey (NHIS) respondents in survey years 1994-2005 who were link-eligible to Social Security Administrative records. Due to the nature of the Supplemental Security Record (SSR) which spans the years 1974 to 2007, I construct my main sample population to include all persons born between 1978 and 1995. I restrict to persons born after 1978 to ensure that the individuals in my sample were exposed to the post-*Zebley* SSI rules. I restrict to persons born before 1995 because this is the last birth cohort for whom I can identify a potential record on the SSR at age 12.[16]

For each individual, I construct five main outcomes of interest which are indicators for whether the individual: (i) applied for SSI between ages 5 and 12, (ii) was awarded SSI between ages 5 and 12, (iii) was awarded SSI between ages 5 and 12 for a mental impairment, (iv) was awarded SSI between ages 5 and 12 for a physical impairment, and (v) was denied SSI between ages 5 and 12.[17] In supplemental analyses presented in the appendix, I disaggregate mental awards into "malleable" mental awards and "nonmalleable" mental awards.[18]

I use applications and awards between ages 5 and 12 as my main outcomes of interest because these are the years in which I expect school-starting-age to affect special education placement. Benson (2018) shows that school-starting-age has the largest effect on special education placement prior to grade 8. If children are using their IEP as documentation of an impairment, I would expect to see school starting age affect disability applications and awards between ages 5 and 12. Table 2.3 presents summary statistics for my main analysis sample: approximately 17.5% of the population identifies as black, non-Hispanic, approximately 60% of the population identifies as White, non-Hispanic, 16% identifies as Hispanic, and 6% identifies as another race. Boys make up 50% of the sample. It is important to note that a small fraction of the population interacts with the SSI program; only 2.7% of the sample applies for SSI between ages 5 and 12

[16] Individuals born before 1978 would not have been exposed to the post-*Zebley* rules prior to age 12, while individuals born in 1996 would be 11 in 2007, the last year of the SSR. Lexus diagrams presented in appendix figures B.1 and B.2 present graphical depictions of the samples.

[17] Regressions where the outcome is awards, awards for mental or physical impairments, and denials are unconditional on having applied for SSI.

[18] "Malleable" mental impairments include: affective disorders, anxiety disorders, personality disorders, conduct disorders, defiant disorders, ADHD, speech impairments, and learning disabilities. "Nonmalleable" mental impairments are as follows: organic mental disorder, schizophrenic disorders, autism, substance abuse, mental retardation, and borderline mental retardation.

Table 2.3: Descriptive Statistics for Persons Born +/- 60 Days of Statewide Cutoff Date

Fraction of Population	+/- 60 Days Around Cutoff Date	60 Days Before Cutoff (Youngest)	60 Days After Cutoff (Oldest)
Applied for SSI Between Age 5 and 12	0.0278	0.0293	0.0263
Denied from SSI Between Age 5 and 12	0.0172	0.0181	0.0163
Awarded SSI for Any Reason	0.0099	0.0105	0.0092
Awarded SSI for Mental Impairment	0.0084	0.0091	0.0076
Awarded SSI for Physical Impairment	0.0012	0.0011	0.0013
Awarded SSI for a Malleable Mental Impairment	0.0035	0.0042	0.0027
Black	0.1747	0.1745	0.1750
White	0.6000	0.6015	0.5985
Non-White, Non-Black Hispanic	0.1619	0.1598	0.1641
Other Race	0.0633	0.0642	0.0625
Male	0.4965	0.4989	0.4940
N	57,260	29,086	28,174
Boys			
Applied for SSI Between Age 5 and 12		0.0373	0.0344
Denied from SSI Between Age 5 and 12		0.0220	0.0200
Awarded SSI for Any Reason		0.0140	0.0132
Awarded SSI for Mental Impairment		0.0123	0.0111
Awarded SSI for Physical Impairment		0.0012	0.0016
Awarded SSI for a Malleable Mental Impairment		0.0065	0.0049
Black		0.1673	0.1740
White		0.6090	0.6017
Non-White, Non-Black Hispanic		0.1600	0.1640
Other Race		0.0636	0.0602
N		14,715	14,158
Girls			
Applied for SSI Between Age 5 and 12		0.0214	0.0185
Denied from SSI Between Age 5 and 12		0.0143	0.0126
Awarded SSI for Any Reason		0.0071	0.0054
Awarded SSI for Mental Impairment		0.0059	0.0043
Awarded SSI for Physical Impairment		0.0009	0.0009
Awarded SSI for a Malleable Mental Impairment		0.0020	0.0007
Black		0.1817	0.1760
White		0.5940	0.5953
Non-White, Non-Black Hispanic		0.1596	0.1641
Other Race		0.0647	0.0646
N		14,371	14,016

Data Source: Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: The data includes all individuals born between 1978 and 1995 who were link-eligible to the Supplemental Security Record. All SSI outcome variables are defined as indicator variables for the outcome occurred to the individual between the ages of 5 and 12. Race categories as defined are mutually exclusive. These are weighted using NHIS person weight adjusted for SSR link-eligibility. See text for details on the weight.

and approximately 1% of the sample has an award for SSI.

2.4 Empirical Strategy

The main relationship of interest in this project is the effect of receiving special education services on the likelihood that a child applies for and is subsequently awarded SSI. This relationship can be described in the following structural equation:

$$SSI_{it} = \alpha + \rho SpEd_{it} + \varepsilon_{it} \tag{2.1}$$

where *SSIist* is an indicator for whether child *i* born in year *t* applied for or is awarded SSI and *SpEd* is an indicator for the child receives special education services with their school. The coefficient of interest is ρ. However, estimating the naive OLS would generate a biased estimate of ρ. First, there may be reverse causality whereby individuals receiving SSI are encouraged to seek out screening for special education services. Second, there is omitted variable bias since children receiving special education services are, on average, more disadvantaged than their regular education peers. For instance, the National Longitudinal Transition Survey conducted in 1987, concluded that 35% of special education children lived with a single parent, 68% had parents with only a high school diploma, and over 26% lived with a household head who was unemployed (Rupp, Davies, Newcomb, Iams, Becker, Mulpuru, Ressler, Romig and Miller, 2006). Further, many special education children live in households receiving federal assistance either through SSI, SSDI, and Medicaid (Cohen, 2007). Thus, children receiving special education services are, on average, more likely to qualify for and apply for SSI due to their higher prevalence of economic disadvantage.

To circumvent the endogeneity of special education, I implement a two-sample fuzzy regression discontinuity design.[19] Using administrative school records from the state of North Carolina, I estimate the increase in special education receipt induced by school entry cutoff dates. This first stage is shown by equation 2.2, where *Youngest* is an indicator that the child was born directly before the school entry cutoff date and *relative age* is the running variable, or

[19]The ideal data set to complete this project in the Instrumental Variables framework would provide a researcher with a nationally representative sample, a child's exact date of birth, his state, administrative records of receipt for special education services, and administrative records for application and receipt of SSI. To the best of my knowledge, no such data set exists.

distance between an individual's date of birth and the school entry cutoff date.[20]

$$SpEd_{it} = \alpha + \beta_{SpEd} Youngest_{it} + f(relative\,age_{it}) + \varepsilon_{ist} \tag{2.2}$$

Next, using National Health Interview Survey (NHIS) linked to Social Security Administration records I estimate the increase in SSI applications and awards induced by school cutoff dates. In particular, I regress indicators for applying for and having an award for SSI on an individual's relative age to the cutoff date. This is shown by equation 2.3.

$$SSI_{it} = \alpha + \beta_{SSI} Youngest_{it} + f(relative\,age_{it}) + \nu_{ist} \tag{2.3}$$

The ratio of the reduced form estimate to the first stage estimate will provide an estimate of the effect of special education on SSI applications and awards as shown by the following expression:

$$\rho = \frac{\beta_{SSI}}{\beta_{SpEd}}$$

I return to the two-sample fuzzy RD estimates in section 2.7. First, I discuss the running variable, a child's relative-age-in-grade. Next, I present first-stage estimates of the increase in special education for children born the month before the cutoff date. Then, I present reduced-form estimates of the increase in disability for children born just before the cutoff date. Last, I present the two-sample fuzzy RD estimates.

2.4.1 Regression Discontinuity Design

I exploit a kindergarten entry eligibility policy, which generates a fuzzy discontinuity in the likelihood a child receives special education services in grade school, to test for spillovers between special education and SSI for children. First, I take as given that kindergarten school cutoff dates generate a sharp discontinuity in the likelihood a child is enrolled in school at age 5, as shown

[20] It should be noted that there is no nationally representative data set that would provide a researcher with exact date of birth and receipt of special education services. While the federal government requires states to report the number of children receiving special education, they do not collect and collate individual level information that would include a child's date of birth and special education status. North Carolina is a large state that closely resembles the nation more broadly as shown in appendix figures B.4, B.5, and B.6.

in Dobkin and Ferreira (2010). The variation in school entry eligibility and subsequent variation in relative-age-in-grade enables an estimate of the causal effect of school entry eligibility on children's subsequent disability applications and awards.

While the school entry cutoff date generates exogenous variation in the age a child is eligible to enter school, the actual distribution of students across grades is complicated by several factors. First, students attending private kindergarten do not adhere to the law and children migrating between states are allowed to remain in the grade they transferred from. An additional concern arises from parents choosing to "redshirt" their child by keeping them out of kindergarten for an additional year. In my data, I am not able to distinguish between each of these mechanisms that would leave a child in a different grade than the cutoff date would assign. Thus, my estimates will be interpreted as intent-to-treat effects for those individuals on the margin of receiving special education services. Specifically, I estimate the effect of being eligible to enter school nearly a year earlier than a similar comparison group.

I utilize the fact that date of birth relative to the statewide cutoff date discontinuously affects the probability that a child is enrolled in special education (Elder and Lubotsky, 2009; Dhuey and Lipscomb, 2010). This strategy assumes that other characteristics associated with child disability evolve continuously through the cutoff date. If this assumption holds, the outcomes of children born just after the cutoff date (those ineligible to enter school) provide reasonable counterfactual outcomes to the children born just before the cutoff date. To implement this strategy, I generate the running variable, which is defined as the distance between the individual's date of birth and the statewide school entry cutoff date. This is shown by the following equation:

$$relative\,age_{it} = school\,entry\,cutoff\,date_t - date\,five_{it} \tag{2.4}$$

where *date five* is the day a child turns 5, where January 1=1 and December 31=365, for individual i for birth cohort t, *school entry cutoff date* is the state-wide school entry cutoff date (September 1=244) faced by individual i for birth cohorts t.[21] Thus, an individual born on August 27th in a state with a cutoff of September 1, would have a *relative age* defined as:

[21] I account for leap years.

$$relative\,age = 244 - 239 = 5$$

By this definition any child with $relative\,age < 0$ is among the oldest in the classroom and any child with $relative\,age \geq 0$ is among the youngest in the classroom.[22] As stated above, I construct my outcomes of interest as indicators for whether the individual applied for and/or was awarded SSI between age 5 and age 12. I then compare differences in the likelihood a child participates in the SSI program between the youngest and oldest individuals within each state and birth cohort. To quantify the discontinuity more precisely, I estimate regressions of the form:

$$\begin{aligned} SSI_{it} = \alpha + \beta_1 Youngest_{it} + \beta_2 Youngest_{it} * relative\,age_{it} \\ + \beta_3 relative\,age_{it} + Controls_{it} + \nu_{it} \end{aligned} \tag{2.5}$$

where *SSI* is the outcome of interest (i.e.: SSI application, SSI award) for individual i born in year t; *Youngest* is an indicator for (being the youngest in in the classroom, having a $relative\,age \geq 0$); *Controls* include indicators for White, Black, and Hispanic, state, and birth cohort. Equation (2.5) specifies a local linear regression, which allows the relationship between relative age and child disability to vary on either side of the discontinuity.[23] The coefficient of interest, β_1, is the size of the discontinuity. That is, β_1 measures the weighted average treatment effect where weights are determined by the probability of being near the state cutoff date or where $relative\,age = 0$ (Lee, 2008).

The RD estimate represents the difference in likelihood of disability for children who are essentially one year younger than their classmates who were born on the other side of the school cutoff date. My main estimates are derived from a local linear specification with a bandwidth of 60 days. Standard errors are clustered at the state level.[24] In all specifications, I present unweighted estimates. However, I show that my estimates are robust to bandwidth selection,

[22] For generality, the reader can think of the youngest in the classroom as those born in the late summer (August) and the oldest in the classroom as those born in fall (September).

[23] Estimates are not sensitive to using the local linear specification. F-tests show that higher order interaction terms do not add explanatory power to the model. Therefore, I present the local linear as the main specification.

[24] Appendix table B.8 shows that estimates are not sensitive to clustering standard errors at the state level.

specification choice, using sample weights, and different assumptions about the correlation of standard errors.

2.4.2 Difference-in-Discontinuity Design

To account for potential differences between children born before and after the cutoff date I leverage the change in eligibility for child SSI due to the 1990 *Sullivan v. Zebley* court decision by estimating a difference-in-discontinuity. The difference-in-discontinuity will estimate the difference in SSI take-up for children born just before the cutoff date to those born just after the cutoff date in years which *Zebley* was in effect and subtract off the difference in the likelihood of an SSI application or award between children born just before and just after the cutoff date for children who applied before the *Zebley* rules were in effect. Formally, I will estimate the following equation:

$$\begin{aligned} SSI_{it} = \alpha + \beta_1 Youngest_{it} * Zebley_t + \beta_2 Youngest_{it} * relative\,age_{it} * Post\,Zebley_t \\ + \beta_3 relative\,age_{it} * Post\,Zebley_t + Controls_{it} + \varepsilon_{it} \end{aligned} \tag{2.6}$$

where *PostZebley* is an indicator for birth cohorts 1978 and later, *Zebley* is the fraction of school years (between age 5 and age 18) an individual experienced after the 1990 *Zebley* decision. Equation (2.7) shows how the Zebley measure is constructed. And all other variables are as defined as above in equation (2.5).[25]

$$Zebley_t = \begin{cases} 0 & if\ t \leq 1971 \\ \frac{Year\,18-1990}{13} & if\ 1972 \leq t \leq 1984 \\ 1 & if\ t \geq 1985 \end{cases} \tag{2.7}$$

The coefficient of interest is β_1, which measures the effect of being the youngest in one's grade on the likelihood a child applies for SSI (or has an SSI award) after *Zebley* liberalized the standard for documentation of child impairments relative to the likelihood a child born just

[25] In the difference-in-discontinuity specification, I have omitted an indicator for *Post Zebley* as it is picked up by the birth cohort fixed effects. *Relative age* and the interaction of *relative age* and *Youngest* are assumed to be zero in the pre-*Zebley* cohorts, thus they are not included in this specification. This assumption was confirmed by estimating the fully saturated model. Similar estimates were obtained in specifications using the continuous measure of *Zebley* in place of the *Post Zebley* indicator.

before the cutoff date applied for SSI before *Zebley* liberalized the standard of documentation. This analysis accounts for potential differences between children born before and after the cutoff date by differencing off the effect of youth-in-grade of the earlier cohorts who did not experience school years under *Zebley*.

2.5 Regression Discontinuity Estimates

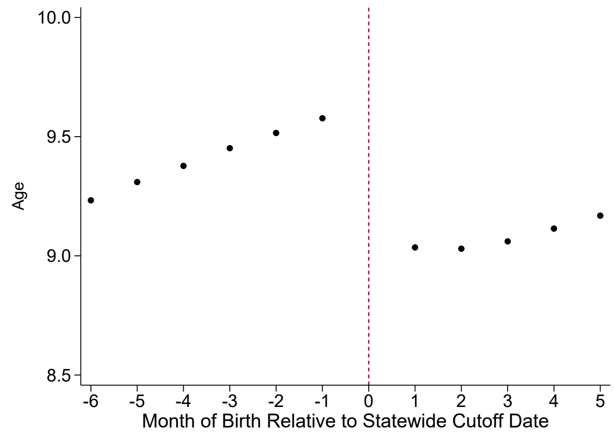
2.5.1 First-Stage Estimates

To begin analysis, I corroborate the previous literature that relative-age affects the likelihood a child is referred to special education services (Elder and Lubotsky, 2009; Dhuey and Lipscomb, 2010). First, in Figure 2.2, I plot the average age of children at the end of their third grade exam by relative age, where relative age is measured as the distance of one's month of birth from the statewide cutoff date. As can be seen in Panel (a), children born the month before the cutoff date are the youngest at the time of the test. In Panel (b), I plot the fraction of children receiving special education services by relative age. A larger fraction of children born the month before the cutoff date, those with relative ages greater than 0, are receiving special education services. Next, I estimate the effect of relative age on special education placement using RD methods analogous to those described in the previous section. Specifically, I estimate the change in likelihood a child receives special education services for children born the month before the cutoff relative to children born the month after the cutoff. I utilize a bandwidth of 5 months; however, estimates are not sensitive to smaller bandwidths. I include child race, birth year, and school fixed effects and cluster standard errors at the child's 3rd grade school.

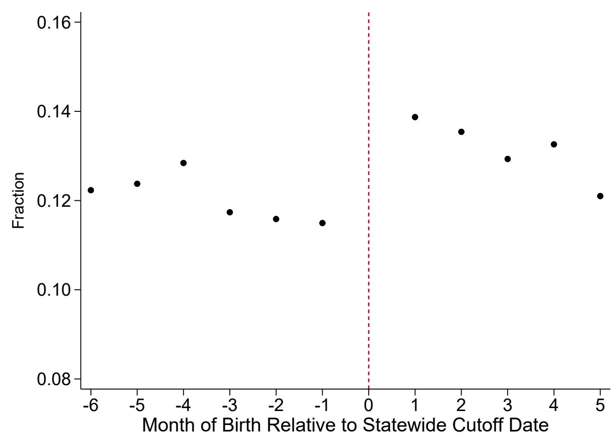
Table 2.4 presents the RD estimates for the estimated increase in special education placement at the cutoff month. I find that children born the month before the cutoff date are 3.28 percentage points more likely to receive special education services at any time between the ages of 5 and 12. Given the base rate of receiving special education services between ages 5 and 12 (12 percentage points), I find that children born the month before the cutoff date are 27% more likely to receive special education services relative to their peers born the month after the cutoff date. Boys born the month before the cutoff date are 3.94 percentage points (or 26%) more likely to be in special education relative to their peers born the month after the cutoff date; while girls born the month

Figure 2.2: Relative Age and Special Education Placement

(a) Age at Grade 3 Exam



(b) Receipt of Special Education Services



Data Source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Includes children in kindergarten cohorts 2006, 2007, and 2008. All October births are dropped from the sample. Panel (a) shows that cutoff date affects the age of children when they take their end of grade 3 exam, or that the variation induced by school cutoff dates does affect the relative age of a child. Panel (b) shows the fraction of children born in each relative month who have received special education services at any time in grades K-6.

before the cutoff date are 2.76 percentage points (or 30%) more likely to be in special education relative to their peers born the month after the cutoff date.

Table 2.4: RD Estimates of the Increase in Special Education Services at the School Cutoff Date

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
Special Education	3.28***	3.94***	2.76***
	(0.37)	(0.59)	(0.46)
Dep. Mean	12.0	15.0	9.0
N	218,519	106,925	111,594

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data come from the North Carolina Department of Public Instruction, school years 2004-2014. The outcome of interest is receipt of special education services between the ages of 5 and 12 (grade K-grade 6). All October births are dropped due to the October 16 cutoff date. This sample is restricted to children who remained in the NC public school system for all years between grade 3 and grade 6.

One may be concerned that North Carolina is not generalizable to the United States. To address this concern, in Figure B.4, I plot the fraction of children receiving special education services in North Carolina against the fraction of children receiving special education services in the remaining US states. North Carolina closely tracks the remaining US states between 1990 and 2000. Between 2000 and 2005 North Carolina places a larger fraction of children in special education, while after 2005 North Carolina places a lower fraction of children into special education. However, North Carolina only deviates from the national average by 0.5%.[26] Further, my first stage estimates are similar to those of Elder and Lubotsky (2009) who utilize the ECLS-K, a nationally representative sample of kindergartners, and find that the youngest in the classroom are 2.5 percentage points more likely to be diagnosed with a learning disability between the ages of 5 and 10. I now turn to the NHIS sample for reduced-form estimates.

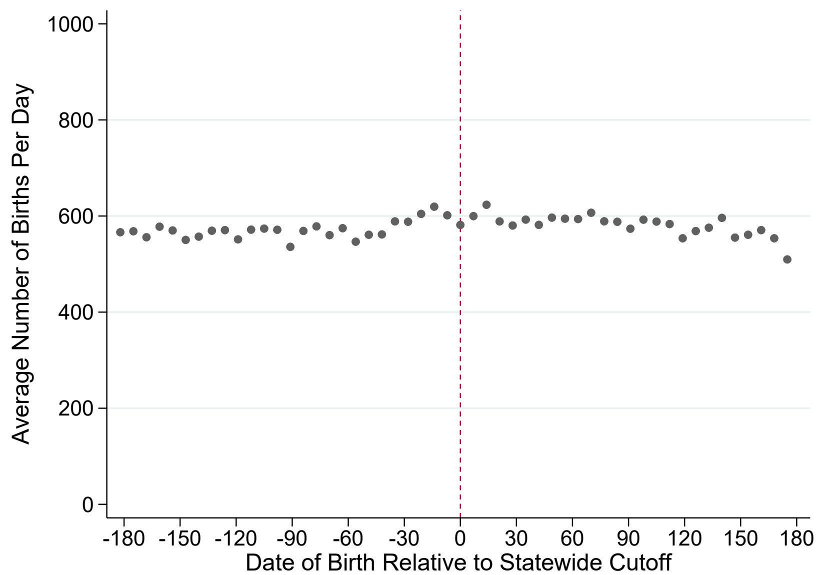
2.5.2 Balance of Covariates/Testing the Identifying Assumptions

To present graphical evidence that the identifying assumptions of the model are satisfied, I test for bunching around the statewide cutoff date. Figure 2.3 displays the average number of births

[26] Appendix Figures B.5 and B.6 further indicate that North Carolina trends similarly to the US average on observable characteristics including the fraction of children on SSI.

by date of birth relative to the statewide cutoff date. Using the NHIS data, births appear relatively uniform. Looking around the cutoff date, there appears to be a slight dip around the cutoff. This likely caused by the reduction in births due to Labor Day, not the statewide cutoff date (Dickert-Conlin and Elder, 2010). Given this distribution of births, bunching around the cutoff date does not appear to invalidate the identification strategy.

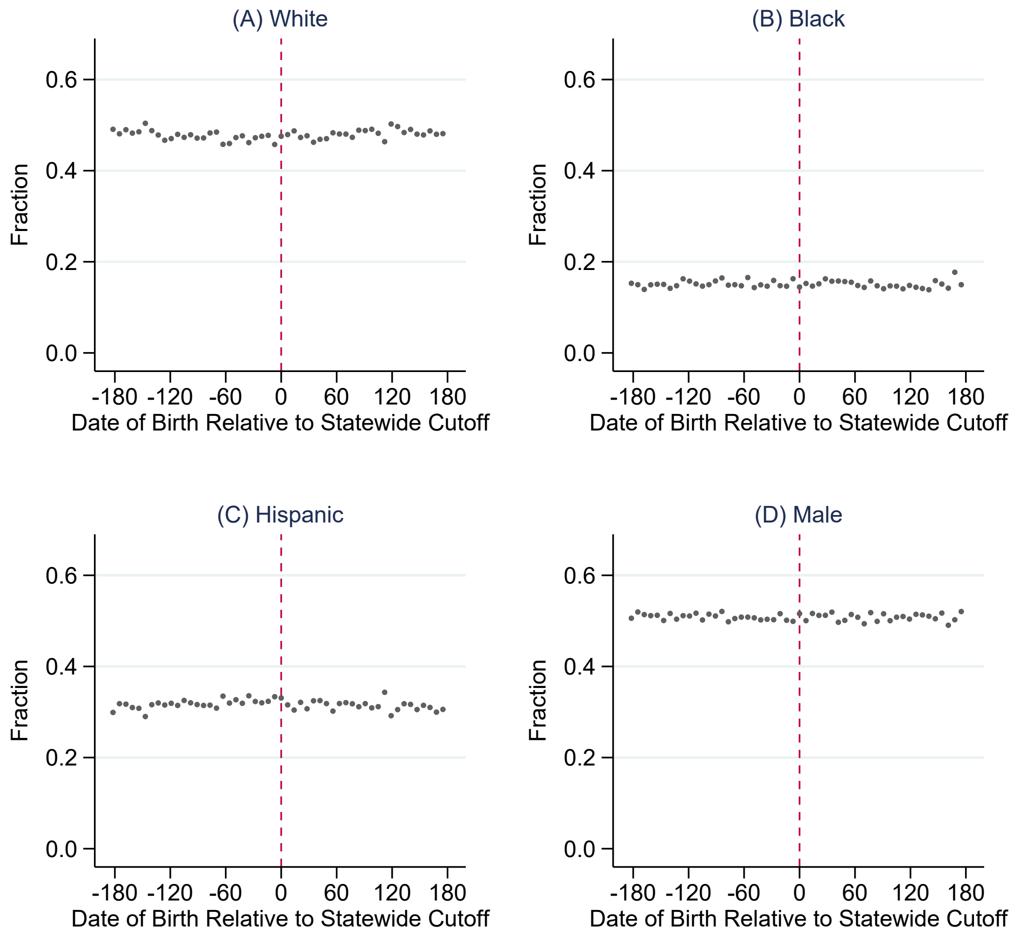
Figure 2.3: Count of Births by Relative Age



Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: The figure above plots the average number of births on each date relative to the statewide cutoff date for individuals who were link-eligible in the NHIS. Each individual's state is defined by their state of birth. State of residence at time of the survey is used for individuals who do not have a state of birth identified in the survey. Statewide cutoff dates vary between states and over time within states. Individuals were matched to the statewide cutoff date in their state of birth for the calendar year in which they turned 5.

Previous work by Buckles and Hungerman (2013) argued that season of birth may be a poor instrument for educational attainment because women who give birth in winter are more likely to be unmarried and/or teenage mothers. Thus, maternal characteristics may explain the differences one finds in educational outcomes due to season of birth instrumental variables. In this setting, use of exact date of birth will circumvent this potential problem. I present graphical and tabular descriptive statistics which show that child characteristics are smooth through the cutoff date. In Figure 2.4, I plot the fraction of persons identifying with each race and gender for each day of birth relative to the cutoff date. There does not appear to be bunching around the cutoff date for any characteristic.[27]

Figure 2.4: Count of Births by Relative Age by Race and Sex



Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: Each individual's state is defined by their state of birth. State of residence at time of the survey is used for individuals who do not have a state of birth identified in the survey. Statewide cutoff dates vary between states and over time within states. Individuals were matched to the statewide cutoff date in their state of birth for the calendar year in which they turned 5.

Summary statistics, density plots, and regression discontinuity estimates together suggest that the underlying assumptions of the model are satisfied. That is, date of birth within the 60

[27]In appendix table B.1 I separately estimate equation (2.5) using each characteristic as the dependent variable. There is no estimated discontinuity in the likelihood of an individual identifying as White, Black, nor Hispanic at the statewide cutoff date.

day window around the cutoff date is essentially random and the individuals do not vary on observed characteristics. Thus, any differences in application and award rates can be attributed to the increased likelihood in receiving special education services.

2.5.3 Graphical Evidence

To graphically view the increase in applications and awards for children born near the cutoff date, I plot the fraction of individuals who applied for or received an award for SSI by date of birth relative to the statewide cutoff date. This is shown in Figure 2.5, where the running variable is aggregated to 30 day bins.[28] As can be seen in panel (a) there is a jump in the fraction of the population applying for SSI at the cutoff date for the post-*Zebley* cohort. As expected, the pre-*Zebley* cohorts do not show a similar discontinuity at the cutoff date. Panel (b) indicates that there is an increase in the fraction of the post-*Zebley* population receiving an award for SSI at the cutoff date.[29] The increase in awards is driven by awards for mental impairments rather than a change in physical impairments (figure 2.5 c).[30]

2.5.4 Reduced-Form Estimates

To confirm the visual evidence of differences in applications and awards, I present estimates of equation (2.5) in Table 2.5 column (1). Each cell contains results from a separate regression. Each specification is a local linear regression with a bandwidth of 60 days, standard errors are clustered at the state level, and dependent means are presented below the the standard error. These estimates are unweighted; thus, they represent the local average treatment effect for the sampled population.

I find that that children born directly before the statewide cutoff date are 0.78 percentage points more likely to apply for SSI relative to their counterparts born directly after the cutoff date. Given the base rate of applications for SSI at 2.59 percentage points, this increased likelihood

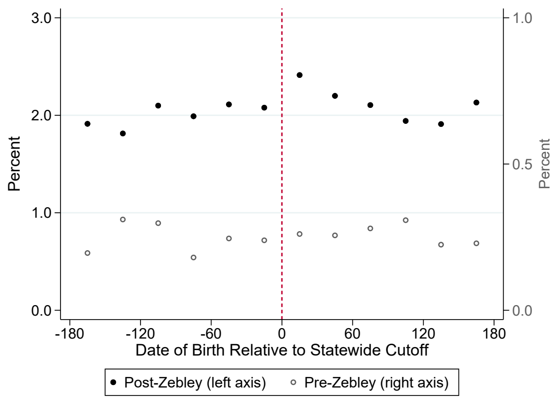
[28] Due to disclosure limitations, data in the form of raw counts has been aggregated to 30 day bins.

[29] Appendix Figure B.7 shows that there is no clear jump in the fraction of the population who receives a denial from SSA at the cutoff date.

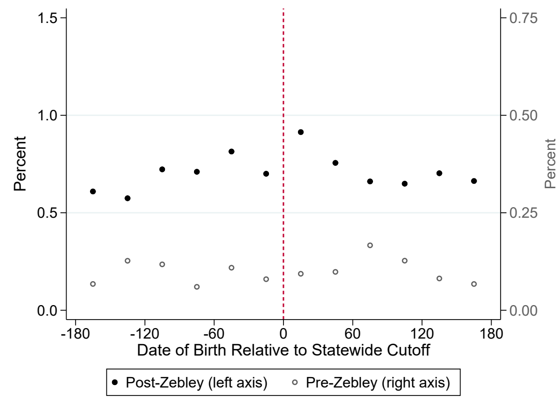
[30] I have not included the following mental impairments in the disaggregated analysis: chronic brain disorder/syndrome; schizophrenic, paranoid, or psychotic disorders; autistic disorders; substance abuse disorders; eating disorders; somatoform disorders; impairment established but no known list code. I do not include mental retardation in the analysis below, however, those results are available upon request.

Figure 2.5: Applications and Awards for SSI by Relative Age

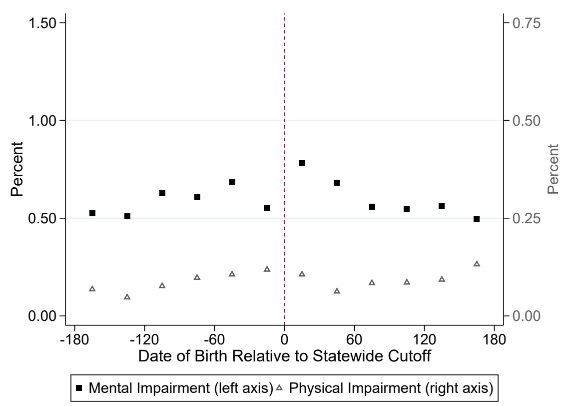
(a) Applications,
pre-*Zebley* and post-*Zebley* cohorts



(b) Awards,
pre-*Zebley* and post-*Zebley* cohorts



(c) Type of Award,
post-*Zebley* cohorts



Data Source: NHIS Surveys 1994-2005, birth cohorts 1957-1995.
Notes: Panel (a) displays the fraction of individuals who have applied to SSI between the ages of 5 and 12 for the post-*Zebley* and pre-*Zebley* cohorts. The data have been aggregated to 30 day bins. Dates to the left of the cutoff represent individuals born after the statewide cutoff date (or the Oldest in the classroom), while dates to the right of the cutoff represent individuals born before the statewide cutoff date (or the Youngest in the classroom). Individuals on the right side of the cutoff date are expected to have a higher likelihood of placement into Special Education; these special education placements are primarily for mental, emotional, and behavioral impairments. Individuals born before *Zebley* are not expected to have a discontinuity at the cutoff date. Panel (b) displays the fraction of individuals who have been awarded SSI between the ages of 5 and 12 for any reason. And panel (c) disaggregates awards by physical and mental impairments for the post-*Zebley* cohorts. The discontinuity in awards is only expected for mental impairments.

in applying for SSI represents a 30% increased likelihood of applying to SSI for those children born directly before the cutoff date. This estimate is statistically significant at the 1% level.

Table 2.5: Regression Discontinuity and Difference-in-Discontinuity Estimates of the Increase in Disability at Statewide Entry Cutoff Date

Dependent Variable	Post-Zebley (1)	Pre-Zebley (2)	Diff-in-Disc (3)	Age 0-4 (4)
Applied	0.78***	-0.03	0.66**	-0.31*
	(0.27)	(0.19)	(0.26)	(0.18)
Dep. Mean	2.59	0.62	2.02	1.62
Award for Any Reason	0.55***	0.01	0.50***	-0.16
	(0.16)	(0.13)	(0.18)	(0.14)
Dep. Mean	0.93	0.24	0.73	0.79
Award for Mental Impairment	0.52***	0.10	0.48**	0.01
	(0.18)	(0.11)	(0.19)	(0.08)
Dep. Mean	0.76	0.16	0.59	0.34
Award for Physical Impairment	0.01	-0.08	0.03	-0.11
	(0.06)	(0.07)	(0.06)	(0.08)
Dep. Mean	0.14	0.06	0.11	0.41
Denied	0.26	-0.03	0.22	-0.14
	(0.24)	(0.14)	(0.22)	(0.12)
Dep. Mean	1.59	0.35	1.23	0.81
N	57,359	23,642	81,001	70,693

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1969-2003. Each cell represents a separate regression. Local linear regressions are specified with a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12 for columns (1)-(3) and for the SSI event occurring between the ages of 0 and 4 for column (4). There is no expected discontinuity in columns (2) and (4).

Next, I test for differences in the likelihood of having ever been denied or awarded SSI between the ages of 5 and 12 for the youngest and oldest children in a grade. I find that the youngest children in the classroom are 0.55 percentage points (or 59%) more likely to receive an award for SSI relative to the oldest children in the classroom. This effect is statistically significant at the 1 percent level. I find no evidence that children born just before the school cutoff date are more likely to have been denied from SSI. The combination of the estimates thus far suggests that the youngest children in a grade are more likely to apply for SSI and to be awarded SSI relative to their older counterparts.

Next, I test whether the increase in awards is driven by mental impairments or physical impairments. Given that physical impairments are difficult to manipulate, I expect that the increase in awards around the cutoff date should be driven entirely by mental impairments. I find that the youngest children are 0.52 percentage points (or 68%) more likely to receive an award for a mental impairment compared to their older classmates. This 0.52 pp increased likelihood accounts for 94% of the increased likelihood in any award.[31] As expected, I find no increase in the likelihood of receiving an award for a physical impairment at the cutoff date; point estimates are zero and are not statistically significant.

2.5.5 Robustness to Bandwidth Choice

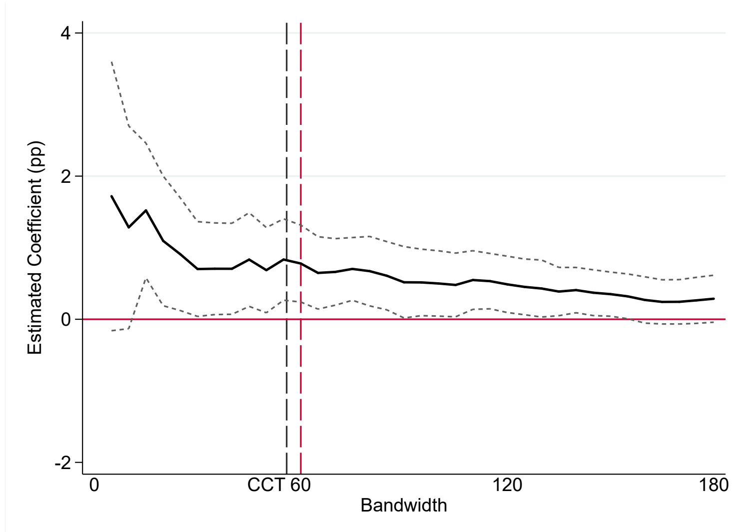
I next assess the robustness of the estimates to different bandwidths. Below, I show that the results are generally similar with and without higher order terms; therefore, I focus the bandwidth robustness exercise on my preferred specification, the local linear regression. Figure 2.6 shows the estimates and 95% confidence intervals for all applications and awards for the full sample. In panel (a), the local linear estimates on the likelihood of applying for SSI remain positive between 0.005 and 0.01 for bandwidths less than 120 days. These estimates are statistically significant at the 95% level. In the bottom panel, the estimates for all awards is robust to bandwidths less than 120 days. The estimates remain between 0.0025 and 0.01. In appendix figure 2.7 I show that the estimates for awards for mental impairments is similarly stable across bandwidths between 5 and 180 days (panel a). Figure 2.7 panel (b) and Figure B.8 show that the estimates for physical awards and denials, respectively, are always close to zero and not statistically significantly

[31] I further disaggregate awards for mental impairments into "malleable" and "unmalleable." Here, malleable is meant to suggest that the impairment could be tied to the child's maturity and personality. I present estimates of the increase in "malleable" mental impairments in appendix table B.2. I find that children are 0.35 percentage points more likely to receive an award for a "malleable" mental impairment relative to their older classmates and this estimate is statistically significant at the 1% level. I find that the difference between all mental awards (0.54 pp) and the malleable mental awards (0.35 pp) is captured by mental retardation, where I estimate a 0.17 pp increased likelihood of having an award for mental retardation for the youngest children. This estimate is not statistically significant at the 10% level. One reason I might see an increased likelihood in awards for mental retardation could be that the test for borderline intellectual functioning is more malleable than an IQ test, or that SSI judges may have been unsure which category best described a child's impairment.

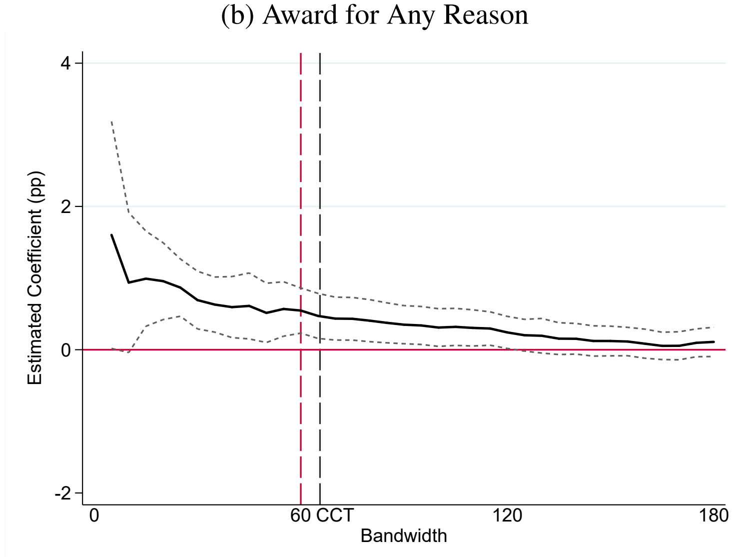
different from zero.

Figure 2.6: Robustness of Estimates to Bandwidth, Applications and Awards

(a) Applications



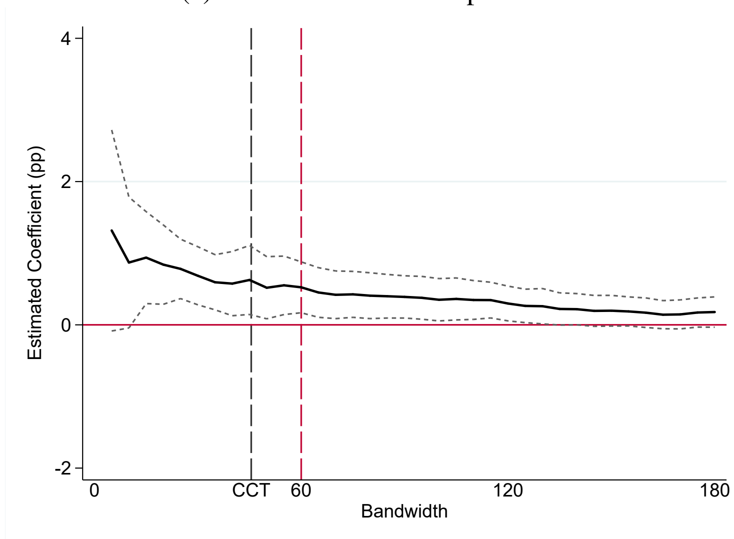
(b) Award for Any Reason



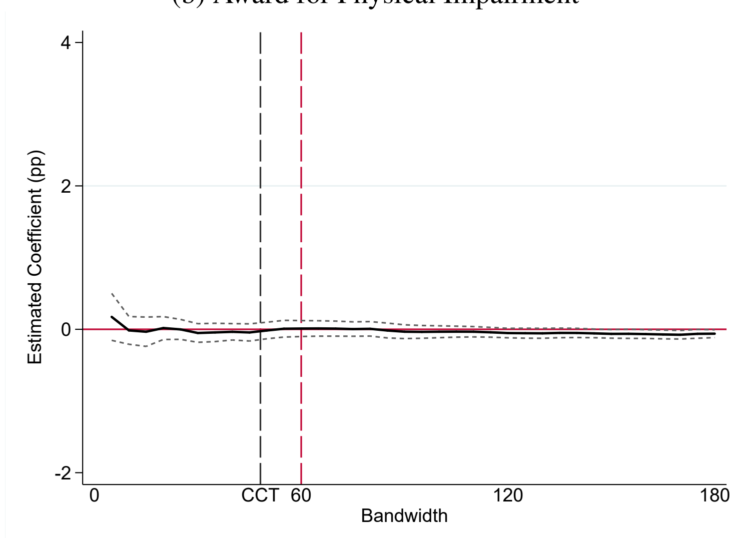
Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths ranging from 5 to 180 days. The local linear regressions are estimated using robust standard errors clustered at the state. The estimates of the full sample are statistically significant for awards and marginally significant for applications for bandwidths less than 120 days. A bandwidth of 60 days is utilized in this project. The bandwidth selected using the method proposed by Calonico, Cattaneo and Titiunik (2014) is also label on the figure (CCT).

Figure 2.7: Robustness of Estimates to Bandwidth, Awards by Type

(a) Award for Mental Impairment



(b) Award for Physical Impairment



Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths ranging from 5 to 180 days. The local linear regressions are estimated using robust standard errors clustered at the state. The estimates of the full sample are statistically significant for awards and marginally significant for applications for bandwidths less than 120 days. A bandwidth of 60 days is utilized in this project. The bandwidth selected using the method proposed by Calonico et al. (2014) is also label on the figure (CCT).

2.5.6 Heterogeneity by Child Sex

To explore how the special education channel to SSI may differentially affect boys and girls, I disaggregate to male and female subsamples. My estimates indicate that special education channel to SSI is larger for boys. Table B.3 presents estimates of equation 2.5 for the sample of boys in column (1) and the sample of girls in column (2). I find that the youngest boys are 1.08 percentage points (or 32.6%) more likely to apply for SSI than their older classmates (statistically significant at the 5% level), while the youngest girls are 0.44 percentage points (or 23.7%) more likely to apply than their older classmates (statistically significant at the 10% level).

Looking at awards, I find that the youngest boys are 0.77 percentage points (or 59.6%) more likely to receive an award for SSI relative to their older classmates and the youngest girls are 0.31 percentage points (or 54%) more likely to receive an award for SSI relative to their older classmates. In terms of mental impairments, I find larger point estimates for boys than for girls. However, I find that given the base rate of awards for mental impairments the effect size is similar for boys and girls. The youngest boys are 0.71 percentage points (or 66%) more likely to have an award for a mental impairment relative to their older classmates, whereas the youngest girls are 0.31 percentage points (or 70%) more likely to receive an award for a mental impairment relative to their older classmates.

These finding makes intuitive sense, given the fact that boys account for the majority of special education placements and make up two-thirds of the SSI child caseload. However, it should be noted that when the sample is disaggregated between boys and girls, I lose statistical power; and thus am unable to estimate effects that are statistically significant at the 5% level for girls. I also cannot rule out the possibility that the effect for boys and girls are statistically significantly different from one another. I focus the subsequent falsification tests and robustness checks on the full sample of boys and girls.[32]

[32] Estimates by child sex subsamples are more sensitive to bandwidth choice. This may be due to decreased statistical power, and thus larger confidence intervals. Despite the loss of precision, estimates are similar across bandwidth choices, yet are not statistically significant at the 5% level. These figures can be seen in Figures B.9 and B.10.

2.6 Falsification Tests and Robustness Checks

One may be concerned that children born directly before the school cutoff date are more likely to exhibit disability even before school starts or that children born before the cutoff date were more likely to be on SSI prior to the 1990 *Zebley* decision. To test this I conduct two falsification tests: first, I test whether children age 5-12 before *Zebley* were more likely to apply for and receive an award for SSI and second, I test whether relative age affects SSI applications and awards prior to age 4. The combination of results from these falsification tests support my assertion that the mechanism through which school starting age affects child disability is through a judge's use of a child's IEP as documentation of the child's impairment. In other words, there is a spillover between special education services and SSI.

2.6.1 Pre-Zebley Birth Cohorts

One may be concerned that children born directly before the cutoff date are more likely to be disabled and this effect would have existed prior to *Zebley*. To test this, I use birth cohorts 1969 to 1977 to test whether children with no exposure to the *Zebley* rules during primary school were more likely to apply for SSI by age 12. Importantly, these children were still more likely to be placed into special education for learning disabilities.[33] Therefore, this is a test of spillovers from the special education program to SSI before *Zebley* opened a direct channel for those spillovers.

First, as shown in Figure 2.5 there are no visual discontinuities in the fraction of individuals applying for and receiving awards for SSI by date of birth relative to the cutoff date for the pre-*Zebley* cohorts (panels a and b). In fact, very few individuals received SSI between ages 5 and 12 prior to *Zebley*. Table 2.5 column (2) corroborates these raw counts by showing that children born directly before the cutoff date were no more likely to apply for SSI nor were they more likely to receive an award for SSI relative to their older counterparts. Point estimates for applications and awards for any reason are essentially zero.

This falsification sample demonstrates that the interaction of special education and SSI drives the relationship between age-in-grade and disability. To more formally test for a change in the

[33] Dhuey and Lipscomb (2010) use the NELS88, with 8th graders surveyed in 1988; this would represent birth cohorts 1974. This sample shows that the youngest children in the grade were still more likely to receive special education relative to their older peers.

likelihood of applying for SSI and receiving an award after the *Zebley* rules come into effect, I estimate a difference-in-discontinuity using the pre-*Zebley* cohorts as controls for the main sample. Column (3) of Table 2.5 presents the coefficient of interest from Equation (2.6).

I find that the youngest children in the post-*Zebley* era are: 0.66 percentage points more likely to apply for SSI, 0.5 percentage points more likely to receive an award for any reason, and 0.48 percentage points more likely to receive an award for a mental impairment relative to their older classmates. These estimates are very similar to the main RD estimates presented in column (1). Thus, the difference-in-discontinuity specification, shows that the mechanism through which relative-youth-in-grade affects disability applications and awards is driven by a judge's use of special education receipt as documentation of an impairment, rather than by a change in the underlying prevalence of child disability for children born around the cutoff date. Children who were young in their grade prior to *Zebley* were just as likely to be placed into special education for mental impairments, however, they were no more likely to apply for or receive an award for SSI.

2.6.2 Age 0 to 4

Next, I test whether relative age affects applications and awards for children prior to school entry—those aged 0 to 4.[34] It is important to note that children can receive special education services prior to age 5. In 2015 of the 6.6 million children receiving special education services, 746,549 were between the ages of 3 and 5 (NCES, 2016). However, children receiving services prior to kindergarten entrance should not be affected by the school starting age, and thus should not have a discontinuous jump in the likelihood of receiving disability benefits at the cutoff date.[35] To test this, I estimate models where the dependent variable is the likelihood of applying for and receiving benefits between the ages of 0 and 4.

As can be seen in Table 2.5 column (4) children born directly before the cutoff date are not more likely to receive an award for SSI relative to their counterparts born directly after the cutoff date. I find that children born directly before the cutoff date are 0.3 percentage points (or

[34]The sample expands to children born between 1978 and 2003, thus capturing all children who turn 4 in or before 2007. See a graphical representation of the sample in the Lexus diagram in figure B.2.

[35]It is possible that pre-schools use the same calendar entrance cutoff date as the public kindergartens. This would suggest that I may find a positive effect in this sample.

19%) less likely to apply for SSI relative to their counterpart born directly after the cutoff date; further, this effect is statistically significant at the 10 percent level. This decreased likelihood in applications is driven entirely by boys who exhibit a 0.63 percentage point decreased likelihood of applying for SSI between ages 0 and 4. However, these decreased likelihoods in applying does not result in any decreased likelihood of receiving an award between the ages of 0 and 4. Overall, I find little evidence that relative age affects the likelihood of a child applying for and receiving disability benefits prior to age 5. This falsification sample provides evidence that school starting age (and the school cutoff date) affects the population of children in school and does not affect children prior to school entry.

Placebo Date Falsification Tests There could be concern that the increase in applications is spurious and equally sized estimates could be found at other nearby dates. To see if this is the case, I conduct two placebo tests. First, I generate "placebo" cutoff dates for weekly intervals 30 weeks before and after the true statewide cutoff date. For each placebo week, I estimate equation (2.5) for the outcomes of interest. I then plot the empirical cumulative distribution function for these placebo estimates using the local linear specification. Figure 2.8 show that the largest estimate for applications and awards are estimated at the true cutoff date. Further, for physical impairments the true cutoff date does not provide distinct estimates from the placebo dates.[36]

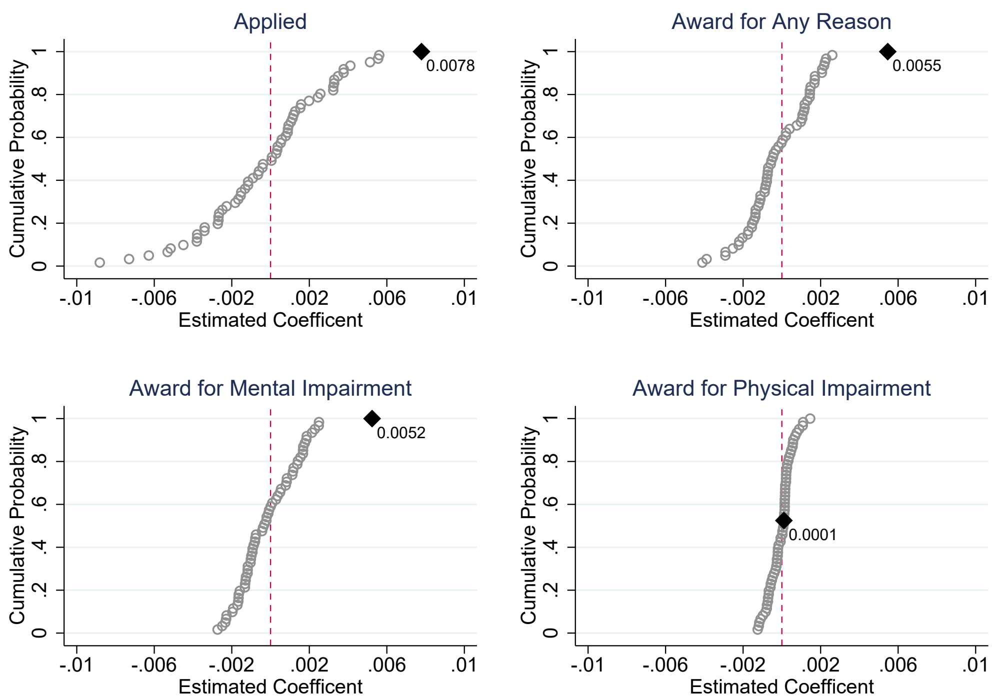
In the second set of placebo dates, I generate placebo cutoff dates that exist for each day between June 1 and December 31. These results are presented in appendix figure B.11. Note that this range includes dates at which there are eligibility thresholds that could affect applications and awards, such as September 1. I find that despite the inclusion of non-placebo dates, the largest estimate remains at the true cutoff date. These placebo tests indicate that the true cutoff date presents an estimated discontinuity that is greater than any random date. Thus, these tests provide additional validity to the instrument.

2.6.3 Model Specification

With RD one may be concerned that the functional form of the relationship between the running variable and the outcomes is misspecified by a local linear regression. Therefore, I esti-

[36]Placebo distributions for denials are presented in appendix Figure B.12.

Figure 2.8: The Distribution of Disability Estimates for Placebo Cutoff Dates +/- 30 Weeks of Actual Cutoff Date



Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: These figures show the conditional density functions of point estimates using each week +/-30 weeks before and after the statewide cutoff date as placebos. The diamond represents the regression discontinuity estimated at the true state cutoff date.

mate analogous regressions using higher order polynomials of the running variable. Table B.4 presents estimates of the effect of relative-youth-in-grade on SSI applications and awards using local quadratic (column 2), local cubic (column 3), and local quartic (column 4) specifications. Looking across columns (2) through (4), the estimates are robust to higher order polynomials. In fact, the local linear specification presents the most conservative estimate for the effect of youth-in-grade on SSI applications and awards.[37] F-tests using the R-squared from each model suggest that higher order terms do not provide substantial additional information to improve the fit of the model. Thus, I selected the local linear regression as the preferred specification.

2.6.4 Sample Weights

In my main results above, I have presented the unweighted estimates. To make my sample nationally representative, I use two different weights. Table B.6 column 2, weights the sample using the NHIS person weight, which does not account for link-eligibility to the SSR. To correct for this, in column (3) I weight my regression models adjusting for a person's link-eligibility to the SSR. I implement a logistic regression to estimate the predicted probability that an individual is link eligible by regressing an indicator for link-eligible on birth-year fixed effects, an indicator for male, race fixed effects, and state fixed effects, as shown in equation 2.8.

$$Link\,Eligible_{ist} = \alpha + \delta_t + \gamma_g + \omega_r + \theta_s + \varepsilon_{ist} \tag{2.8}$$

Then I multiply the predicted probability of an individual being link-eligible with the predicted probability of being in the NHIS sample.[38] As can be seen, weights do not drastically affect the estimates. Similar to the higher order specifications, the unweighted estimates tend to provide the most conservative estimate.

2.6.5 Dissipation of Effects Between Ages 13 and 17

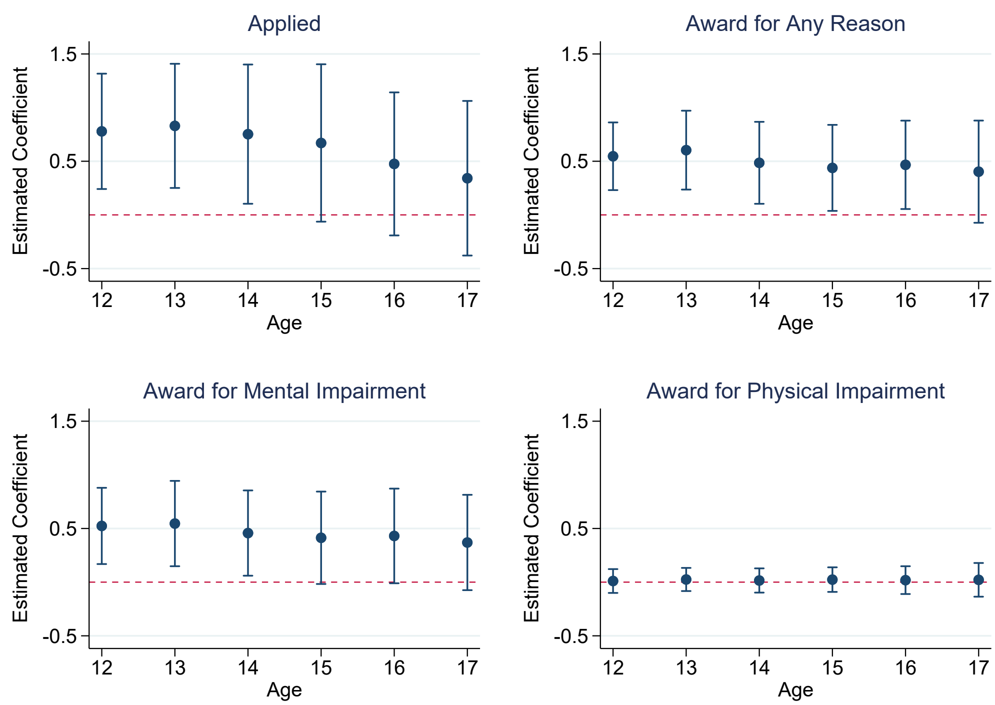
While my main specification focuses on the likelihood of applying for and receiving benefits between ages 5 and 12, I examine the full trajectory of children's application and award behavior

[37] Analogous robustness tables for gender subsamples can be found in appendix table B.5.

[38] In practice, I multiply the link-eligible weight with the NHIS person weight, and estimate my regressions using pweights.

between ages 5 and 17. It is important to note that overall acceptances for SSI decline after age 13, similar to special education.[39] Figure 2.9 plots the estimated increase in SSI applications and awards at the cutoff date for each age between 12 and 17. As can be seen, the estimated effect of school entry cutoff date on SSI peaks at age 13 and diminishes with each subsequent age. By age 15, there is no statistically significant difference in application rates between children born on either side of the statewide cutoff date. However, awards for any reason persists through age 16, where children born directly before the cutoff are more likely to have an award.[40]

Figure 2.9: Dissipation of RD Estimates for Ages 13 to 17



Data Source: NHIS Surveys 1994-2005, birth cohorts included varies with age.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals for applications and awards between ages 12 and 17. Each age represents the likelihood of applying or receiving an award between age 5 and the given age. The local linear regressions are estimated using robust standard errors clustered at the state with a bandwidth of 60 days. The effect of the statewide school cutoff date dissipates after age 13. Reasons for why these effects dissipate between ages 13 and 17 are discussed in the text.

There are two reasons why the effect of school entry cutoff date on SSI may dissipate over time. First, sample construction necessarily reduces both the sample size and the fraction of

[39]See figure 1.7.
[40]Analogous figures for boy and girl subsamples are presented in appendix figures B.13 and B.14.

children experiencing the *Zebley* SSA rules.[41] As sample size declines, the statistical power at each age falls, therefore, precision of estimates decreases. Second, the effect of school-starting-age on special education placement diminishes for each grade after grade 8 (Benson, 2018). Thus, one may expect that school-starting-age would not affect application and award decisions for children at older ages. Given the limitations of the data, I cannot disentangle these two hypotheses. Understanding this diminishing effect is an important area of future research, as it has significant implications for the relationship between special education and SSI.

2.7 Two-Sample Fuzzy RD Estimates

I now return to estimating the direct effect of special education on SSI. Combining the first stage and the reduced form estimates, I obtain the two-sample fuzzy RD estimate for ρ.[42] There are two assumptions required for validity: (1) there is a strong first stage and (2) the exclusion restriction is not violated. First, as shown by the first-stage estimates, relative-age-in-grade affects an individual's likelihood of receiving special education. Thus, the first stage exists and is strong. The exclusion restriction requires that the instrument, relative-age in grade, affects SSI only through special education. This is fundamentally untestable. A violation of the exclusion restriction would require that an individual's relative age affects SSI eligibility through some channel other than special education.

Examples of a violation might include that a child's date of birth affects maternal labor supply, subsequently affecting family income, and thereby affecting a child's financial eligibility. However, a violation of this manner would downward bias my estimates. Children born directly after the cutoff date would have depressed maternal labor supply, lower family income, and thus would be more likely to be financially eligible for SSI. A second violation may arise due to a Buckles and Hungerman (2013) type argument where maternal characteristics vary by season a birth. Buckles and Hungerman find that children born in July and August are more likely to have better maternal characteristics and come from wealthier families. If a violation of this manner

[41] For each age, the sample includes one fewer birth cohort, because I restrict analysis to individuals who obtain the oldest age in the sample by 2007 (see figure B.10 panel (a) for cohorts included for each age). Additionally, as the sample is expanded to older ages, my sample contains earlier birth cohorts, who on average experienced fewer years under the *Zebley* SSA rules.

[42] The two-sample fuzzy RD estimate is calculated as the ratio of the reduced-form and first stage coefficients. Standard errors were calculated using the delta method under the assumption of zero covariance between the first-stage and reduced form estimates as in Dee and Evans (2003).

occurred it would bias my estimates downwards.

Table 2.6: Two-sample Fuzzy RD Estimates of Special Education on SSI

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
Applied	23.48***	27.44*	15.94*
	(8.64)	(14.07)	(9.10)
Dep. Mean	2.59	3.31	1.85
Award for Any Reason	16.46***	19.29**	11.23*
	(5.22)	(7.68)	(5.80)
Dep. Mean	0.93	1.29	0.57

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. The two-sample fuzzy RD estimate is calculated as the ratio of the reduced-form and first-stage coefficients. Standard errors were calculated using the delta method under the assumption of zero covariance between the first-stage and reduced-form estimates as in Dee and Evans (2003).

Table 2.6 presents two-sample fuzzy RD estimates. I find that special education induces a 23 percentage point increase in the likelihood of applying for and a 16 percentage point increase in the likelihood of receiving an award for SSI. In this individual level regression, receipt of special education services goes from zero to one. We can think about this as a change in the fraction of the population receiving special education services as going from 0 to 100%. Thus, a 1 percentage point increase in the fraction of children receiving special education services induces a 0.23 percentage point increase in the fraction of children applying for SSI and a 0.16 percentage point increase in the fraction of the child population with an award for SSI. Given that approximately 1.6 percent of children have an SSI award, this represents a 10% increase in the SSI roll or an increase of 118,400 additional SSI awards. Taking the average SSI award of $662, I estimate that a 1 percentage point increase in the fraction of children receiving special education services will cost an additional $940 million annually in SSI payments.

I find that these effects are driven primarily by boys. A 1 percentage point increase in the fraction of boys receiving special education services induces a 0.27 percentage point increase in the fraction of boys who apply for SSI and a 0.19 percentage point increase in the fraction of boys who have an SSI award. While a 1 percentage point increase in the fraction of girls receiving special education services induces a 0.16 percentage point increase in the fraction of girls who apply for SSI and an 0.11 percentage point increase in the fraction of girls who have an award for SSI.

To address the question posited by Aizer et al. (2013), how much have rising rates of special education contributed to the rise in SSI?, I calculate the fraction of the total growth in SSI that can be attributed to the special education channel. First, note that between 1990 and 2013, the fraction of the child population enrolled in special education services increased from 11.4% to 12.9%, or 1.5 percentage points, and the fraction of the population with an SSI award increased from 0.48% to 1.8%, or 1.3 percentage points. Thus, the fraction of growth in SSI that can be attributed to rising rates of special education can be calculated as follows:

$$Fraction\,of\,SSI\,Growth\,Explained\,by\,SpEd = \frac{\Delta SpEd * 0.16}{\Delta SSI} = \frac{1.5 * 0.16}{1.3} = 0.18$$

Thus, approximately 18% of the increase in SSI can be attributed to rising special education rates.

2.8 Discussion and Conclusion

The last 30 years have seen dramatic growth in the SSI program. A 1990 expansion to program eligibility, which allowed children to qualify for SSI using school counselor and teacher identification of child disability, reduced the cost to apply and increased information on individual eligibility. Eliminating these large barriers to program participation was expected to increase program take-up. Despite the potential connection between special education and SSI, very little evidence exists on how the two programs interact to provide services for disabled children. Aizer, Gordon and Kearney (2013) find a positive correlation between the rate of special education in a state and the child SSI caseload in the state. However, they found no correlation between the rising rate of special education and applications to SSI, suggesting that special education would affect the SSI roll only by reducing the likelihood that a child's application is denied. Contrarily, I find that receipt of special education induces an increase in both SSI applications and awards.

Using a fuzzy RD design, I estimate that children born just before the school cutoff date are 0.78 percentage points more likely to apply for SSI relative to children born just after the school cutoff date. I find no evidence that children born just before the cutoff date are more likely to have their application denied. Rather children born just before the cutoff date are 0.55 percentage points more likely to have an award for SSI, and 0.52 percentage points more likely to have an award for a mental impairment relative to children born directly after the school cutoff date. I find no increase in awards among groups in which there is no expected interaction between school-starting-age and disability: including physical impairments and children too young to enroll in school. Further, I find no discontinuity in applications and awards for children in which there is no expected special education to SSI channel, i.e.: those enrolled in primary school prior to *Zebley*. My estimates are robust to a wide range of specification choices and are significantly larger than nearby placebo dates.

Two-sample fuzzy RD estimates indicate that a 1 percentage point increase in the fraction of children receiving special education services induces a 0.23 percentage point increase in the fraction of children who apply for SSI and a 0.16 percentage point increase in the fraction of children with an award for SSI. Given that only 1.6% of the child population has an SSI award, my estimates indicate that a 1 percentage point increase in the fraction of children receiving special education services induces a 10% increase in the SSI caseload. My estimates indicate that approximately 18% of the growth in the child SSI caseload can be attributed to rising rates of special education and spillovers between these two programs.

Identifying a causal channel between special education and SSI is important for three reasons. First, SSI is indispensable for the families receiving payments. SSI for children has been shown to significantly reduce the prevalence of poverty (Duggan and Kearney, 2007). This is, in large part, due to the fact that SSI is means-tested, and children receiving SSI are significantly more disadvantaged than the average child (Duggan, Kearney and Rennane, 2015; Rupp et al., 2006; Deshpande, 2016b). On average, SSI payments account for nearly half the income of the child's family and provide important stabilization of income effects (Rupp et al., 2006; Deshpande, 2016a). Further, Coe and Rutledge (2013) show that children who received SSI due to the *Zebley* expansion had greater labor force attachment in adulthood and were less likely to claim welfare as an adult. Thus, the special education channel to SSI enables access to resources for these highly disadvantaged youth.

Second, there are unintended consequences of local special education screening policies which affect the federal disability roll. Since the true prevalence of child disability is unknown, it's possible that children born before the cutoff date are being over-diagnosed, children born just after the cutoff date are under-diagnosed, or more likely a combination of both. Regardless, declining special education enrollment since 2005 will unintentionally affect the SSI roll. Schools facing budget shortfalls are faced with increasing pressures to keep children out of special education services.[43] Recent works suggest that children with mental impairments are the most likely to be affected when a school faces financial incentives to change the population of students receiving special education services (Cullen, 2003). Therefore, there may be unintended consequences of declining special education enrollments for low-income children on the margin of qualifying for SSI.

Third, it implies that special education provides a previously unaccounted for benefit to low-income children eligible for SSI benefits. While special education students comprise 13% of all students and receive about 18% of all funding for public schools (USDE, 2015), little evidence exists on the effectiveness of special education due to the challenge of identifying a suitable control group (Hanushek, Kain and Rivkin, 2002). Regardless of where the literature stands on the costs and benefits of special education, this study identifies a benefit provided to low-income children eligible for services. Future research examining the benefits of special education should consider the option-value of applying for SSI benefits.

[43] In 2004, Texas implemented a rule limiting special education services to 8.5% of the population (Rosenthal, 2018). Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, Utah, and Wisconsin now offer vouchers for children with disabilities to attend private schools, which are not bound to uphold the requirements of IDEA (EdChoice, 2018, The Wrightslaw Way, 2010). Further, Winters and Greene (2007) argue that the advent of special education voucher programs will disincentivize schools from diagnosing children with disabilities since each student identified as needing special education services will become a voucher-eligible student who is able to take their public-education dollars away from the public school.

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CHAPTER 3

THE IMPACT OF THE EARNED INCOME TAX CREDIT ON CHILD DISABILITY[1]

3.1 Introduction

In 2017, approximately 1.2 million children between the ages of 0 and 18 received Supplemental Security Income (SSI) for a childhood disability (Social Security Administration, 2017a). Despite the generous cash benefits, the youth on SSI tend to be particularly disadvantaged with high rates of school dropout, low employment rates in early adulthood, and high arrest rates (Deshpande, 2016b; Deshpande, 2016a). A recent literature has shown that the Earned Income Tax Credit (EITC) reduces the prevalence of child poverty, increases child health, and increases children's academic achievement (Hoynes and Patel, 2014; Short, 2014; Dahl and Lochner, 2012). I test whether exposure to a larger EITC payment as a child affects the likelihood that a child receives disability payments between the ages of 15 and 18. Using a difference-in-differences analysis, I show that exposure to a larger EITC benefit as a child reduces the likelihood of receiving SSI payments between the ages of 15 and 18. I find no evidence that the channel through which SSI diminishes is through improvements in child health.

Growth in the child SSI caseload has been linked to the 1990 liberalization of medical eligibility criteria for children; state incentives, which encouraged states to shift children from traditional welfare to SSI (Kubik, 1999; Kubik, 2003), and rising rates of special education (Benson, 2019). Various aspects of the SSI program raise the possibility of interactions with state, local, and other federal programs. For example, SSI often provides automatic enrollment in Medicaid, SSI recipients have particularly high participation in SNAP, and evidence suggests fiscal spillovers between AFDC and SSI (Duggan, Kearney and Rennane, 2015). However, few studies have examined how aspects of the current social safety net interact with the SSI program. Since its introduction in 1975, the EITC has grown to be one of the biggest and least

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controversial elements of the US safety net (Nichols and Rothstein, 2015).

To estimate the effect of the EITC on SSI participation, I utilize individual level data from the American Community Survey (ACS) and implement a difference-in-differences strategy. I follow Bastian and Michelmore (2018) and generate a measure of EITC exposure which reflects the cumulative maximum credit available to that individual based on their year of birth, state of residence, and the number of qualifying children within the household. This strategy leverages three sources of variation: first, federal expansions in the EITC for various family structures in 1990, 1994, and 2009; second, the introduction of state EITCs; and third, changes in family structures across ages.

There are several reasons why the EITC may interact with the SSI program. The EITC is one of the largest cash transfer programs in the United States. It has been shown to reduce child poverty (Nichols and Rothstein, 2015) and improve child health (Hoynes, Miller and Simon, 2015). Both improved child health and reduced child poverty should decrease a child's likelihood of being awarded SSI. However, receipt of the EITC may increase information about safety net programs more generally and the SSI program in particular. Further, an increase in cash-on-hand from the EITC may increase the likelihood that a child visits a doctor and has a disability identified and treated. Therefore, the theoretical predictions between EITC and SSI are ambiguous.

I find that exposure to an additional \$1000 from the EITC each year between the ages of 0 and 18 reduces SSI participation by 0.34 percentage points (26%). Consistent with the hypothesis that cash- on-hand affects children's disability claims, I find that children are most responsive to additional financial support at older ages; exposure to a larger EITC between the ages of 13 and 18 leads to the largest reduction in SSI claiming between the ages of 15 and 18. An additional \$1000 each year between 0 and 5 has no effect on SSI, exposure to an additional \$1000 each year between 6 and 12 reduces the likelihood of receiving SSI by 0.10 percentage point, and exposure to an additional \$1000 each year between the ages of 13 and 18 reduces the likelihood of receiving SSI by 0.25 percentage points. As a robustness to the potential endogeneity of state EITCs , I restrict cumulative exposure to the federal EITCs and I find qualitatively similar results. However, point estimates are slightly larger, where I find that exposure to an additional \$1000 each year between 0 and 18 in the federal EITCs reduces SSI participation by 0.45 percentage points (35%). Again, exposure to the EITC at older ages has a largest impact on reducing SSI participation between ages 15 and 18.

I examine two potential channels through which exposure to the EITC reduces participation with the SSI program. First, the EITC provides additional resources to low-income households. This "income effect" could lead to improvements in long-run outcomes through improved health of the child. Second, mothers may increase their labor supply along either the intensive or extensive margin as a response to the EITC. This increase in a family's earned income may make families ineligible for the SSI program due to the assets and earnings tests. I find no evidence that the channel through which exposure to the EITC reduces SSI participation is through improved health. I find that exposure to an additional $1000 in EITC each year does not reduce the likelihood a child reports a cognitive impairment, has a small and insignificant effect on the likelihood a child reports a vision impairment, and increases the likelihood a child reports a physical impairment by 0.12 percentage points (12%).

I similarly find no evidence supporting the second hypothesis, that the EITC increases family income through increased labor supply of the mother. Exposure to a larger EITC does not increase the likelihood that a mother is employed. Ninety-five percent confidence intervals rule out increases in maternal labor supply greater than 0.2 percentage points (0.3%). Given that exposure in the later years has the largest effect of disability recipiency, these estimates seem to suggest that additional cash-on-hand affects a family's decision to apply for child disability, despite no change in the child's eligibility for SSI. This means that families may use the EITC and SSI as substitutes.

I contribute to the literature in three ways. First, I expand the nascent literature examining spillovers among social safety net policies (Elwell, 2019; Levere, Orzol, Leininger and Early, 2019). Importantly, I identify how a federal policy like the EITC can reduce the child disability roll. This finding is particular important to policymakers and the Social Security Administration. Second, I augment the growing literature on how the EITC affects children (Dahl and Lochner, 2012 and Bastian and Michelmore, 2018). Similar to Bastian and Michelmore (2018) these results indicate that the EITC has long-lasting impacts on children growing up in low-income households, with declines in child disability at older ages. The ability of the EITC to reduce child SSI participation suggests that anti-poverty programs are an effective avenue to reduce the

SSI disability roll. Third, I expand the literature on the child SSI program. In particular, I find that antipoverty programs, like the EITC, have the ability to reduce the child disability roll. This finding is important because previous literature has found little empirical evidence that other safety net programs interact with the child SSI program[2]

A persistent concern of policymakers and the Social Security Administration has been the rapid growth of the child SSI program (Aizer, Gordon and Kearney, 2013). A descriptive report by Aizer, Gordon and Kearney (2013) highlight five factors that may contribute to the growing SSI caseload. First, there may be a broader set of diagnoses in the population at large; second, there may be spillovers between health insurance and SSI; third, there may be spillovers between special education and SSI; fourth, there may be an increase in child poverty leading more children to turn to disability; and lastly, there may be a reduction of SSI terminations. Research on these five hypotheses are ongoing, and this project tangentially speaks to the fourth hypothesis.[3]

In 2015 the EITC disbursed over $68.5 billion to 28.1 million tax filers (Faulk and Crandall-Hollick, 2018), making it one of the largest safety net programs in the US. Research unequivocally finds substantial positive impacts of the EITC on low-income families. For example, the EITC has been shown to increase labor force participation of single mothers (Eissa and Liebman, 1996), increase family income (Dahl, DeLeire and Schwabish, 2011), and reduce the likelihood a child lives in poverty (Hoynes and Patel, 2014). To the extent that the EITC reduces the prevalence of child poverty it may be associated with decreases in child disability.

This paper proceeds as follows. Section 3.2 provides background information about the SSI program and the EITC. Section 3.3 discusses the data and sample. Section 3.4 presents the empirical strategy. Section 3.5 outlines the change in disability for children differentially exposed to the EITC. And Section 3.6 concludes with a discussion.

[2] For example, see Levere et al. (2019).

[3] Aizer et al. (2013) find no evidence that increased diagnoses in the population at large has contributed to increases in the SSI caseload. Levere et al. (2019) find that Medicaid expansions did not induce an increase in SSI applications or awards. Benson (2019) finds that rising rates of special education substantially contributed to the rising SSI caseload.

3.2 Institutional Framework

3.2.1 Supplemental Security Income for Children

Supplemental Security Income (SSI) is a federally funded program designed to support blind and disabled individuals who have limited financial resources (SSA, 2014). Originally SSI was a small program primarily serving the elderly population. However, in 2017, the federal government spent approximately $9.6 billion on SSI cash benefits to 1.2 million children (Social Security Administration, 2018a). In terms of total spending, SSI is now second only to Medicaid among means-tested programs (Duggan and Kearney, 2007). Growth in child SSI was facilitated by the 1990 Sullivan v. Zebley Supreme Court decision, which liberalized the standards for a child to qualify for SSI based on mental or emotional disorders (Kubik, 1999; Duggan, Kearney and Rennane, 2015).

To qualify for SSI the child must be under 18 (or have developed the disability prior to turning 18), not be working or earning more than $1070 a month, have a physical or mental condition that results in "marked and severe functional limitations," and the child's conditions must be expected to be disabling for a minimum of 12 months or result in death. In the child SSI program, income and assets of the parent, referred to as "deemed income," are used to determine both financial eligibility and the monthly benefit amount.[4] In 2015, the federal benefit rate (FBR), or the maximum monthly benefit level, was $733 for individuals and $1100 for couples. While, the average monthly payment varies greatly by age group, children receive the most generous payments (Duggan, Kearney and Rennane, 2015).[5]

SSI for children has been shown to significantly reduce the prevalence of poverty. Using an event study design Duggan and Kearney (2007) show that enrolling a child in SSI increases household income by $400 per month without a significant offset from other transfer programs or earnings. This is, in large part, due to the fact that SSI is means-tested, and children receiving SSI are significantly more disadvantaged than the average child. Most children receiving SSI

[4]Deductions for parents and other children living in the home are subtracted from the deemed income. In 2017, a single parent family with one disabled child and one non-disabled child could earn approximately $3000 in monthly income and still qualify for a small child SSI payment (Social Security Administration, 2018d).

[5]An SSI recipient's monthly benefit falls below the FBR if a family member has income (either earned or unearned). Child eligibility is based on the same asset limit and income eligibility as adults and includes both the child's and parent's assets.

payments live in single mother households, and fewer than 1/3 live with both parents (Duggan, Kearney and Rennane, 2015; Rupp, Davies, Newcomb, Iams, Becker, Mulpuru, Ressler, Romig and Miller, 2006; Deshpande, 2016a). Further, half of SSI children reported living in a household with at least one other individual with a disability (Rupp et al., 2006). On average, SSI payments accounted for nearly half the family's income and provide important stabilization of income effects (Rupp et al., 2006; Deshpande, 2016a). Despite the benefits of SSI, youth on SSI remain particularly disadvantaged. According to Deshpande (2016b) SSI youth with mental impairments have high school dropout rates around 45% and arrest rates around 28%.[6] Further, former SSI youth have employment rates between 30 and 50 % when they reach adulthood (Deshpande, 2016b). Thus, understanding ways in which government programs may reduce the prevalence of child disability is important.

Similar to this study, Levere et al. (2019) examine an interaction between the child SSI program and a large social safety net program, Medicaid. They find that expansions in children's health insurance, through Medicaid and SCHIP, neither increased nor decreased the SSI roll. However, for the subset of states that did not automatically grant Medicaid to SSI child recipients, expansions to Medicaid reduced SSI applications. They find that a 10% increase in the share of children covered by Medicaid reduced SSI applications by 5%. My work builds on the literature of spillovers between social safety net programs for low-income and disabled children, where I find that expansions to the EITC significantly reduced the likelihood that a child reports income from SSI.

3.2.2 The Earned Income Tax Credit

The EITC is a refundable tax credit that provides an annual earnings subsidy to low-income working parents. Eligibility for the EITC is based on family structure and family earned income. A primary feature of the EITC is presence of a "qualifying child" in the household. A child qualifies if the child is under the age of 19 and resides in the household for at least half of the tax year.[7] The second eligibility criteria is earned income. To qualify for the EITC earnings must be positive yet remain below a threshold that varies with family size. In 2014, this maximum

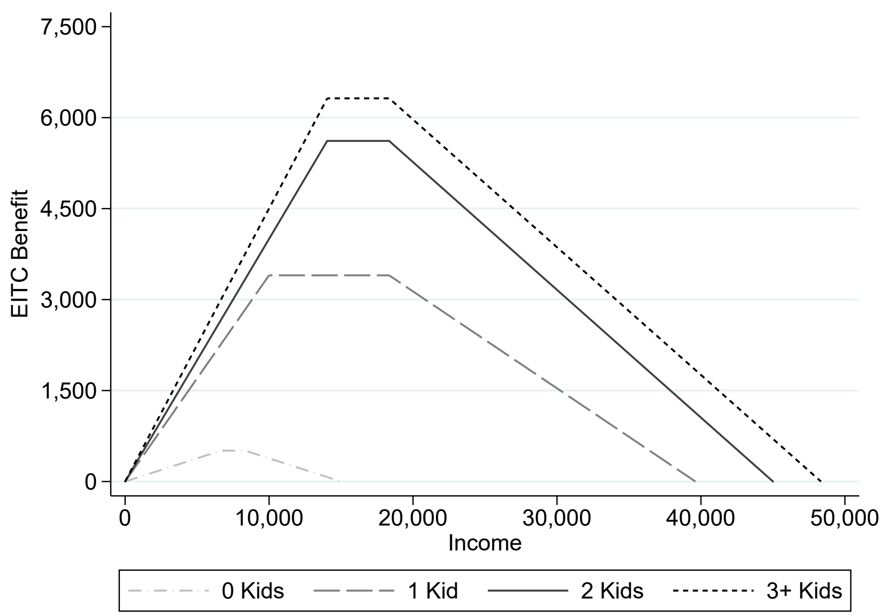
[6] Importantly, mental conditions including ADHD, speech delay, and autism spectrum disorder have accounted for nearly all the expansion in the SSI caseload since 1998 (Aizer, Gordon and Kearney, 2013; Deshpande, 2016b).

[7] A child can also qualify if they are under 24 and a full-time student or permanently disabled.

threshold was $48,378 per year for a family with two children. Take-up of EITC is high: between 80 to 86% of eligible families take-up the EITC, and take-up is highest among families with children (Scholz, 1994; Jones, 2014).

Since its inception in 1975 the EITC has received bipartisan support with expansions authorized by both democratic and republican congresses. The EITC structure features a phase-in range, in which benefits increase as a function of earnings, a plateau region in which benefits are constant with respect to earnings, and a phase-out region, in which benefits decrease as a function of earned income (see Figure 3.1 for the 2018 EITC benefit schedule). For a comprehensive history of the EITC see Nichols and Rothstein (2015), here I provide a brief summary of the relevant changes to the EITC. In 1975 the EITC provided a 10% subsidy rate with a maximum credit of $400 and a 10% phase-out rate for incomes over $6000; this credit was only eligible to families with children. In 1990, a more generous schedule for families with two or more children was introduced. The largest expansion of the EITC occurred in the mid-1990s.

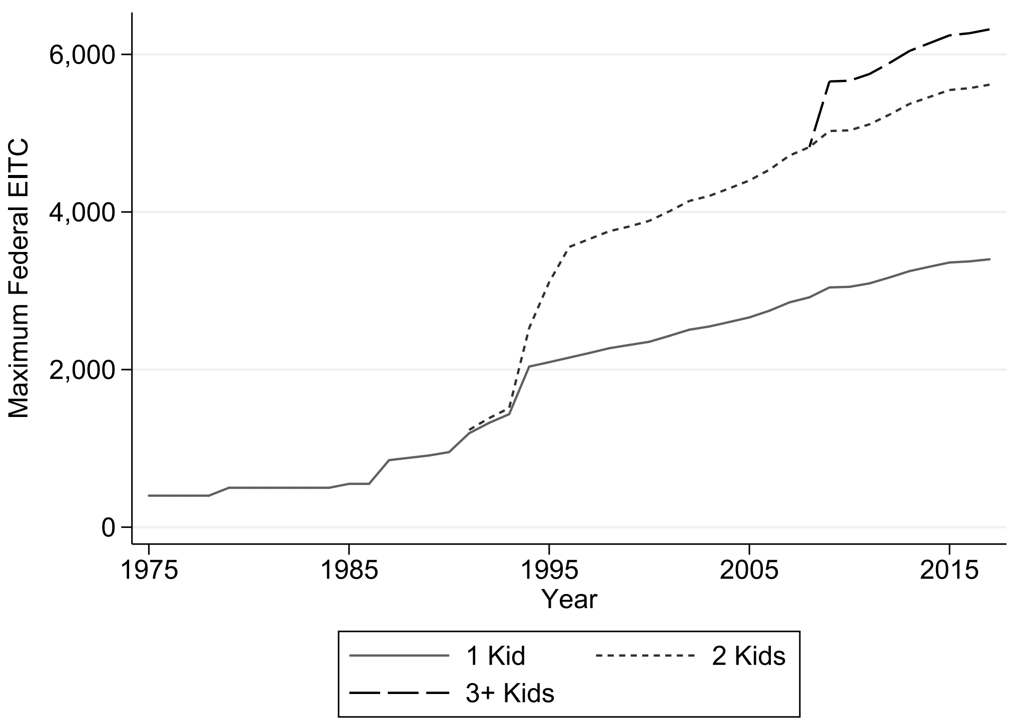
Figure 3.1: 2017 Earned Income Tax Credit Schedule by Number of Children



Data source: Authors calculations based on data provided by the Tax Policy Center, Urban Institute and Brookings Institution.

In 1994, the EITC was increased sharply particularly for families with two or more children for whom the credit was roughly doubled. In 2002, a separate schedule was introduced for married couples and single parents, however these schedules were not vastly different from one another. The latest change in the EITC schedule occurred in 2009 with the American Recovery and Reinvestment Act (ARRA). The 2009 expansion increased maximum credits and introduced a more generous schedule for families with three or more children. Figure 3.2 presents a graphical representation of the changes in the maximum EITC for different family sizes by tax year. In addition to federal expansion of the EITC, 29 states and the District of Columbia have introduced their own EITCs. Typically these refundable credits (although some are non-refundable) are a percentage of the federal EITC. In 2017, state credits ranged from 4% of the federal EITC for a one child family in Wisconsin to 85% of the federal EITC in California. Table 3.1 presents a summary of state EITCs, which were introduced prior to 2017.

Figure 3.2: EITC Maximum Benefit, By Tax Year and Number of Children ($2013)



Data Source: Author's calculations based on the maximum credit reported in Table 1 by Nichols and Rothstein (2015).

The EITC has been shown to have large impacts on low income families, primarily through increased labor supply of single mothers (Eissa and Liebman, 1996). Further, expansions of the EITC in the mid-1990s were associated with a large decline in child poverty rates. See Nichols

Table 3.1: State Earned Income Tax Credit

State	Year EITC Introduced (1)	Generosity (as fraction of Federal EITC) (2)
California	2015	85*
Colorado	2015	10
Connecticut	2011	30
District of Columbia	2000	40
Delaware	2005	20
Iowa	1989	17
Illinois	2000	10
Indiana	1999	9
Kansas	1998	17
Louisiana	2007	3.5
Massachusetts	1997	23
Maryland	1987	26
Maine	2000	5
Michigan	2006	6
Minnesota	1991	24
Nebraska	2006	10
New jersey	2000	30
New Mexico	2007	10
New York	1994	30
Ohio	2013	10
Oklahoma	2002	5
Oregon	1997	8
Rhode Island	1986	15
Virginia	2004	20
Vermont	1988	32
Wisconsin	1989	34

Data source: National Conference of State Legislatures.
Notes: States not listed in the table do not have a state EITC, these include the following: Alaska, Alabama, Arkansas, Arizona, Florida, Georgia, Hawaii, Idaho, Kentucky, Missouri, Mississippi, Montana, North Carolina, North Dakota, New Hampshire, Nevada, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Washington, West Virginia, and Wyoming. *California provides a state EITC of 85% the federal EITC for families earning up to ½ the phase-in region, approximately $7000.

and Rothstein (2015) for a comprehensive summary on the literature linking EITC expansions and child poverty. Further, introduction of state EITCs have been associated with increases in the likelihood that families below the poverty line in one year have earnings above the poverty line in the subsequent year (Neumark and Wascher, 2001). Overall, the US Census Bureau estimates that the EITC has lifted 4.7 million children out of poverty (Nichols and Rothstein, 2015).

The literature consistently finds that children benefit greatly from the EITC. Hoynes, Miller and Simon (2015) find that the EITC led to a reduction in low birth weight children, Dahl and Lochner (2012) find that additional income from the EITC increases child test scores by 6 percent of a standard deviation, and Bastian and Michelmore (2018) find the EITC increases the likelihood of high school graduation, the likelihood of college enrollment, and early career earnings. Due to the anti-poverty capabilities of the EITC and its ability to improve the health and academic outcomes of children, it is likely that the EITC may diminish the likelihood a child participates in the SSI disability program.

Owing to the EITC's administration through the tax code, EITC eligibility does not directly impact participation in other social programs such as SNAP, TANF, or SSI. Further, SSI payments do not count towards earned income, nor does the EITC refund count towards the SSI income limit. Therefore, there is no mechanical relationship between the EITC benefit amount and individual eligibility for SSI. However, Nichols and Rothstein (2015) note that there may be important interactions between EITC take-up with participation in other programs. Importantly, Caputo (2006) finds that food stamp receipt tripled the odds of filing for the EITC, but finds no statistically significant correlation with SSI or TANF. In contrast, Jones (2014) finds that EITC receipt conditional on eligibility is negatively associated with adult SSI participation. I contribute to this literature by testing whether exposure to a larger EITC as a child reduces the likelihood a child participates in the SSI program between the ages of 15 and 18. Similar to Jones (2012), I find that exposure to a larger EITC reduces participation with the SSI program.

3.3 Data

The main source of data utilized comes from the IPUMS-American Community Survey (ACS). The ACS is the largest household survey in the United States. Conducted by the Census Bureau, the ACS surveys approximately 3 million households annually. The ACS is a mandatory survey with responses collected via mail or through an online portal. Of particular benefit, the ACS collects information on all members within the residence and includes questions pertaining to income sources including SSI for all household members over the age of 15.

I use survey years 2001 through 2017, and each survey year represents approximately 1% of the US population.[8] To construct the main sample, I first generate a measure for whether the individual is a dependent child. Dependent children are defined as being younger that 19, or under age 25 and attending college. I further restrict to individuals who are listed as the child, stepchild, or grandchild of the head of household. The ACS is a repeated cross-section; therefore, I use an individual's family composition at the time of the survey to back out the number of qualifying children within the household for each year of the child between the ages of 0 and 18. I then use information on the maximum federal and state EITC from the Tax Policy Center and the National Conference of State Legislatures to generate the main treatment measure, EITC exposure. EITC exposure is the sum of all maximum federal and state credits an individual was eligible to receive starting at age 0 through the survey year. Credit amounts are indexed to 2013 dollars and annualized by dividing by the child's age. Figure 3.3 depicts the variation in cumulative EITC for the sampled population.

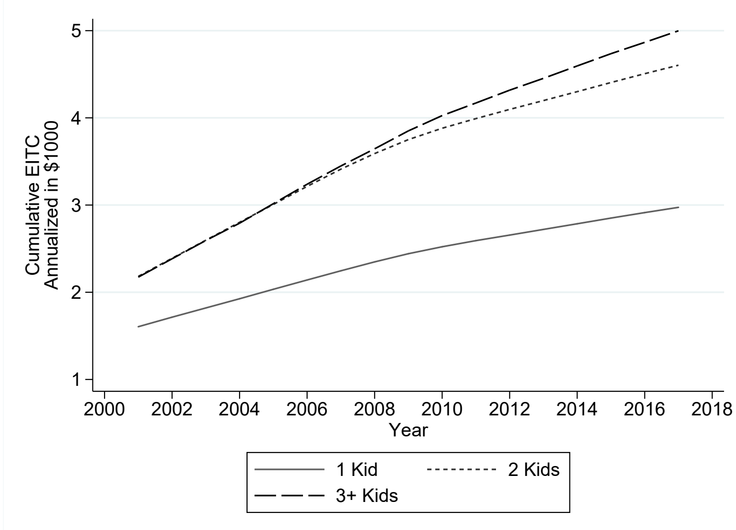
Because information on SSI is only available for individuals over the age of 15, I restrict the final analysis sample to be all dependent children aged 15-18. This leaves me with 2,131,221 observations. These individuals experience various levels of the EITC, depending on their year of birth, state of residence, and family size at each age. The main benefit of the ACS is its large sample size as well as its high response rate. However, due to the survey time frame, 2001-2017, a major limitation of the ACS is the inability to view the contemporaneous change in disability claims during the large federal expansion in the early-1990s. Thus, I supplement my analysis using the IPUMS-Current Population Survey (CPS). I construct the CPS sample in an analogous fashion to the ACS. Using survey years 1970-2017, I limit the sample to dependent children born between 1952 and 2002. The final sample includes 463,361 individuals.

Table 3.2 presents descriptive statistics for the main analysis sample. Approximately 1.3% of all children report income from SSI, 4.4% report a cognitive impairment, 1.4% report a visual

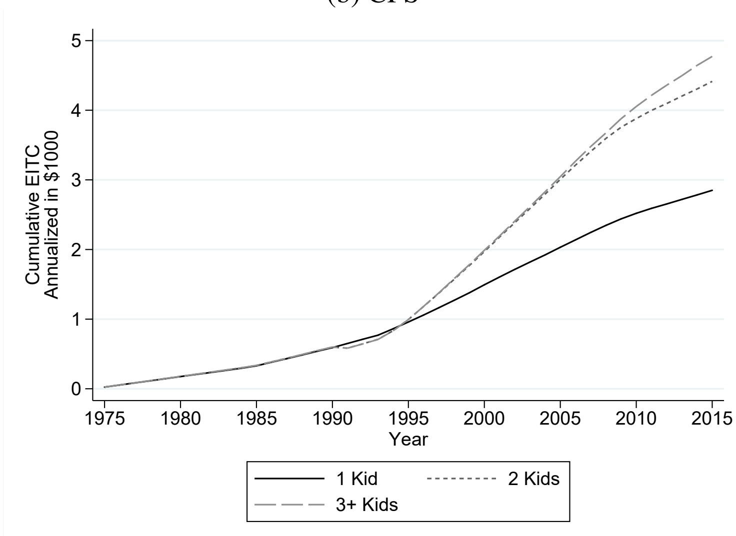
[8] In years 2001-2004 only 0.4% of the US population is represented. Results are robust to limiting analysis to survey years 2005-2017.

Figure 3.3: Cumulative Exposure to Federal EITC, by Year and Number of Children in Household

(a) ACS



(b) CPS



Data source: American Community Survey years 2001-2017 (panel a) and Current Population Survey years 1975-2017 (panel b).
Notes: Cumulative exposure to the maximum state and federal EITC is constructed as the sum of maximum federal and state credits available to a child's family based on year of birth, number of qualifying children within the household, and state of residence.

impairment, and 1% report a physical impairment. Approximately 14% live in a household with a reported income that falls below 100% of the federal poverty level (FPL). And 65% have a mother that is currently in the labor force. Race has been defined as mutually exclusive indicators, thus Hispanic represents an individual who reported they are non-white, non-black and Hispanic. The majority of the sample is white (70%), 14% are black, and 19% are Hispanic.

Table 3.2: Summary Statistics

Variable (Fraction)	Mean (1)	Std. Dev (2)
SSI Income	0.013	0.114
Cognitive Impairment	0.044	0.204
Visual Impairment	0.014	0.116
Physical Impairment	0.010	0.100
Below FPL	0.143	0.350
Mom Employed	0.654	0.476
Adult Population Disabled	0.024	0.008
Adult Population Unemployed	0.050	0.014
Black	0.145	0.352
Hispanic	0.189	0.392
White	0.706	0.456
Female	0.483	0.500
Low Educated Mother	0.437	0.496
Single Mother	0.333	0.471
Number of Children in Household	2.045	0.789
Cumulative Exposure to Federal EITC (Annualized in $1000)	3.287	0.964
Cumulative Exposure to Federal and State EITC (Annualized in $1000)	3.478	1.098
Contemporary Maximum Federal EITC ($1000)	4.443	1.141
Contemporary Maximum Federal and State EITC ($1000)	4.755	1.328
N	2,131,221	

Data source: American Community Survey 2001-2017.
Notes: Data include children born between 1983 and 2002 who were between the ages of 15 and 18 at the time of the survey and are classified as the child, stepchild, or grandchild of the head of household.

Children in my sample typically have a sibling. Approximately 30% of the sample has no sibling, 40% have one sibling, and 30% have more than one sibling; the average household thus has 2 children. The average child is exposed to a federal EITC of $3,870 each year, during the survey year the average EITC maximum credit is $4,086 indicating that the EITC is more generous in later years. Summary statistics for the CPS sample are similar and are reported in Appendix Table A1.

3.4 Empirical Strategy

The ideal experiment to determine how receipt of EITC affects child SSI awards would be to randomly assign some children to families that receive the EITC and some children to families that do not receive the EITC and comparing SSI awards among those from the treated to those in the control group. This is not feasible for financial and political reasons. Therefore, I use exogenously induced changes to the EITC schedule that differentially affect children in different states, born in different years, with greater or fewer qualifying dependent children in the household to identify the effect of the EITC on SSI recipiency.

To test whether the EITC affects the likelihood of claiming child SSI, I follow Bastian and Michelmore (2018) by defining exposure to the EITC as the maximum federal and state credit a child's family could potentially receive given their state of residence, family size, and the tax year. This measure is independent of actual family income and parental marital status. For each individual in the sample, EITC is cumulated from the year an individual is age 0 to the year an individual appears in the ACS at age 15 to 18. The value of EITC exposure varies over time for an individual based on federal and state policy changes to the EITC as well as changes in family size. For instance, a first born child will receive the federal and state maximum credit for a one child family until the year a second child is born. Once a second child enters the family, both children will be assigned the maximum federal and state credit of a two child family for that year and state. Once the oldest child turns 19, the second child will be assigned the maximum federal and state credit for a one child household until he turns 19.

Variation in EITC exposure stems from three primary source: (1) the year the individual was born, (2) the state an individual resides, and (3) the number of children in the household in each year. I leverage the exogenous variation in federal and state expansions for children residing in various family structures in a difference-in-differences framework. Specifically, I estimate the following equation:

$$SSI_{its} = \alpha + \omega_1 Cumulative\ EITC_{(0-18)} + \gamma X_i + \tau_t + \theta_s + \sum_{a=0}^{A} \pi_{it}^{a} + \rho_c + \varepsilon_{its} \qquad (3.1)$$

where SSI_{its} is an indicator for whether individual i, residing in state s receives income from the SSI program in year t. $Cumulative\ EITC_{(0-18)}$ is the treatment measure, which is the annual cumulative maximum state and federal EITC the individual was eligible to receive based on year of birth, state of birth, and number of siblings for each year since age 0 (in thousands of dollars). Year, state, and birth-year fixed effects are included and denoted as τ_t, ϑ_s, and ρ_c, respectively. I further control for number of children in the household each year for ages $a \in (0, A)$, denoted by π_{its}^{a}. Individual characteristics including gender, race, an indicator for whether the mother has a high school diploma or less, and an indicator for a single mother are included in the vector X_i. I further control for the statewide unemployment rate and statewide adult SSI disability rate.[9]

The coefficient of interest in equation (3.1) is ω_1, which represents the impact of an additional \$1000 each year since age 0 on subsequent child disability. In the fully specified model variation in the maximum EITC is driven by differences in EITC generosity across and within states over time, family size, and year of birth. Inclusion of year, state, and age-by-siblings fixed effects will control for any fixed differences across states, years, or family size. Thus, identification primarily comes from three sources: (1) federal expansions to the EITC schedule for larger families in 1990, 1994, and 2009, (2) introduction of state EITC across years, and (3) changes within a family structure across ages.

The main identifying assumption in equation (3.1) is the following: absent the expansions of the maximum credit in the EITC, children with varying family sizes across all states would have the same trends in disability, ceteris paribus. That is, conditional on state and year fixed effects, trends in child disability among children in single child households is an accurate counterfactual for trends among children with siblings. I test the validity of this assumption using event-study analysis. Further, specifications in which state variation is removed and exogenous variation is limited to changes in the federal EITC schedule and family composition show qualitatively and quantitatively similar results.

To test the hypothesis that early childhood investment in health drives the reduction in child

[9]The unemployment and disability rate are calculated from the ACS or CPS as the fraction of adults aged 19-64 in each state and year who are unemployed or claim SSI income, respectively.

disability, I disaggregate the cumulative exposure measure to three age ranges (0 to 5, 6 to 12, and 13 to 18). If the main mechanism through which EITC affects child disability is through improved health at younger ages, I would expect to find the largest decreases in SSI claiming from exposure at earlier ages. If however, cash-on-hand affects disability decisions, I may expect that exposure to a larger EITC at older ages has the largest impact on disability claims. To disentangle these two hypotheses, I estimate the following model:

$$
\begin{aligned}
SSI_{its} = \alpha + \beta_1 EITC_{(0-15)} + \beta_2 EITC_{(6-12)} + \beta_3 EITC_{(13-A)} \\
+ \gamma X_i + \tau_t + \theta_s + \sum_{a=0}^{A} \pi_{it}^{a} + \rho_c + \varepsilon_{its}
\end{aligned}
\tag{3.2}
$$

where $EITC_{i(0-5)}$, $EITC_{i(6-12)}$, $EITC_{i(13-A)}$ represents the annual cumulative maximum EITC the individual was eligible to receive when the child was between the ages 0 to 5, 6 to 12, and 13 to age at time of survey, respectively. All other variables are as shown in equation 3.1. The coefficients of interest are β_1, β_2, and β_3 which represents the impact of an additional \$1000 each year when the child was 0 to 5, 6 to 12, and after 13 on subsequent disability income. As in equation 3.1 exogenous variation stems from differences in EITC generosity across and within states, family size, and year of birth.

While the ACS does not allow a difference-in-differences analysis of the 1994 EITC expansion, it does allow a difference-in-differences analysis using the 2009 ARRA expansion to the EITC which increased generosity for families with three qualifying children relative to families with 2 qualifying children. I estimate the following difference-in-differences specification:

$$
SSI_{its} = \alpha + \delta_1 Post\ 2009_t * (3 + kids)_{its} + \delta_2 (3 + kids)_{its} + \gamma X_i + \tau_t + \theta_S + \rho_c + \xi_{its} \tag{3.3}
$$

where SSI_{its} is an indicator that individual i, residing in state s, reports receipt of SSI in year t. The indicator $3 + kid$ takes the value of 1 if the child resides in a household with 3 or more qualifying dependents (the child has 2 or more siblings). $Post2009$ is an indicator that the survey year is after 2009. Similar to equation (3.1) I include year, state, and birth year fixed effects, and a vector of individual controls. The coefficient of interest is δ_1, which measures the difference-in-differences estimate for children with two or more siblings after the small federal expansion in 2009. This model is restricted to families with 2 or more qualifying children, thereby dropping all one child families.

The model is identified by comparing changes in SSI receipt when the federal expansion in 2009 is introduced across families with three or more children compared to families with two children. Inclusion of year, state, and number of qualifying children fixed effects picks up any fixed differences in SSI claims by state, year, or household size. The underlying assumption is that absent the expansion to the EITC children with two or more siblings and children with one sibling would trend similarly in disability claims after 2009. I test this assumption using an event study.

Next, I follow Hoynes, Miller and Simon (2015) where I utilize the contemporaneous maximum federal and state credit available to an individual given the year, their family size, and their state of residence. Specifically, I estimate the following equation:

$$SSI_{itsk} = \alpha + \lambda_1 maximum\ credit_{tsk} + \delta_k + \gamma X_i + \tau_t + \theta_S + \rho_c + \varepsilon_{itsk} \tag{3.4}$$

where SSI_{itsk} is an indicator for whether individual i, living in state s, with k qualifying children in the household reports income from SSI in year t. *Maximum Credit* is the dollar value (in thousands) of the maximum federal and state EITC available to an individual based on the federal schedule (in which generosity is determined by the year), the number of qualifying children in the household, and the individual's state. I include number of qualifying children fixed effects which are denoted by δ_k. The coefficient of interest is λ_1, which measures the effect of an additional \$1000 increase in EITC benefit for someone with k qualifying children in the household. Similar to my main analysis, identification comes from multiple sources, (1) federal expansions to the EITC based on the year and (2) introduction of state ETICs.

The largest expansion to the EITC occurred between 1993 and 1994. Data limitations in the ACS do not allow me to examine the immediate effect of the EITC expansion on children in 1994. Therefore, I supplement analyses using the CPS. I implement an analogous difference-in-differences strategy to equation (3.3) using the large federal expansion in 1994 on child SSI.

Specifically, I estimate the following equation:

$$SSI_{its} = \alpha + \delta_1 Post\ 1994_t * sibling_{is} + \delta_2 sibling_{is} + \gamma X_i + \tau_t + \theta_S + \rho_c + \xi_{its} \tag{3.5}$$

where SSI_{its} is an indicator that individual i, residing in state s, reports receipt of SSI in year t. $Sibling$ is an indicator for the individual lives in a household with two or more dependent children (the child has one or more siblings), $Post1994$ is an indicator that the survey year is after 1994. Again, I include year, state, and birth-year fixed effects, and a vector of individual controls. The coefficient of interest is δ_1, which measures the difference-in-differences estimate for children with one or more siblings after the large federal expansion in 1994.

The model is identified by comparing changes in SSI receipt when the federal expansion in 1994 is introduced across families with two or more children compared to families with only one child. Inclusion of year, state, and number of qualifying children fixed effects picks up any fixed differences in SSI claims by state, year, or household size. The underlying assumption is that absent the expansion to the EITC children with siblings and children without siblings would trend similarly in disability claims after 1994. I test this assumption using an event study. Finally, I estimate models analogous to equations 3.3 and 3.4 using the CPS data.

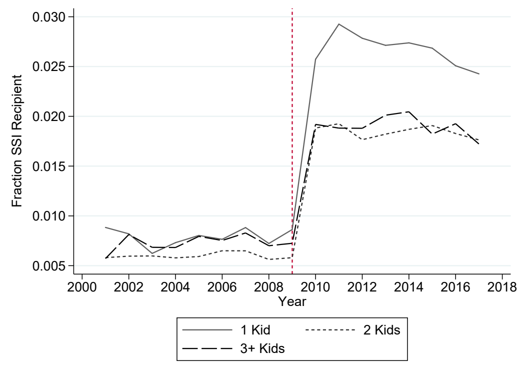
3.5 Results

To begin analysis, I first test the underlying assumptions of the model. Figure 3.4 plots the fraction of children on SSI by family size and year. As can be seen in panel (a) there was a large change in the fraction of children reporting SSI income in ACS survey year 2010 relative to year 2009. However, it is not differentially reported for one child, two child or three plus child families. The difference-in-differences analysis using the 2009 ARRA expansion in the EITC will difference off the change in reporting for those years. As can be seen, children living in one child families report the highest rates of receiving SSI, and children living in a three child family are more slightly likely to claim SSI income than children in two child families. After the 2009 expansion in the EITC, the gap between two child families and three child families diminishes, and the fraction of one child households that report SSI income falls.

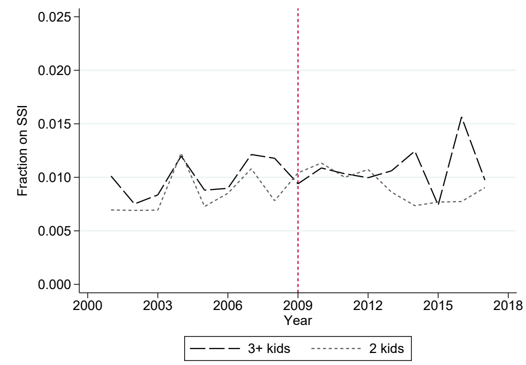
Panel (b) plots the fraction of SSI recipients using the CPS. As can be seen the change in

Figure 3.4: Supplemental Security Income, by Year and Number of Children in Household

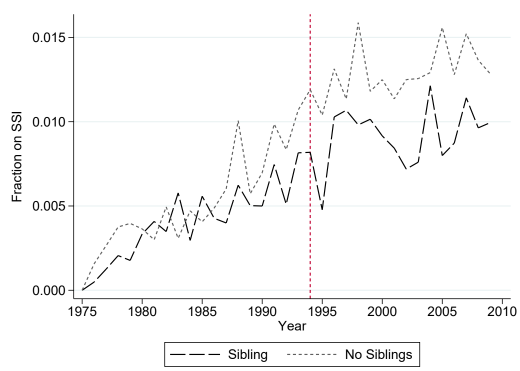
(a) ACS



(b) CPS 1995-2017



(b) CPS 1975-2005



Data source: American Community Survey years 2001-2017 (panel a), Current Population Survey years 2001-2017 (panel b), and Current Population Survey 1975-2010 (panel c). Notes: SSI is measured as whether the individual (aged 15-18) claims income from Supplemental Security Income. In 2009, the EITC was expanded for families with 3 or more qualifying children relative to families with 2 qualifying children. It is important to note that there appears to be a change in the fraction of families reporting SSI income in 2010 for ACS respondents. This does not appear in the CPS. There was no change in the ACS questionnaire in 2010, this creates a data challenge for analysis using the ACS.

SSI reporting in 2010 does not appear in the CPS. Again, in the years immediately following ARRA, the gap between three child families and two child families diminishes slightly. Panel C compares one child families to two plus child families from 1975 through 2005. As in the ACS, one child families report the highest rates of claiming SSI income. Importantly, disability rates between one child and two plus child families trend similarly prior to 1994. After 1994, the gap between one child families and two child families increases slightly, indicating that a larger EITC to two child families may reduce their disability take-up even lower. However, these series are noisy given the smaller sample size and how few children are on the disability roll. To concretely explore the validity of the design, I estimate event study models.

3.5.1 Event Time Estimates

I begin with event study models of the two largest expansions to the EITC, OBRA 93 and ARRA 2009. In practice, I estimate equation (3.3) with a full set of year effects interacted with the indicator for sibling (for OBRA93) and an indicator for multiple siblings (ARRA 2009). I then plot the year by sibling(s) interactions. This allows examination of the pre-trends. Event study models for ARRA 2009 are of the following form:

$$SSI_{its} = \alpha + \sum_{t=2001, t\neq 2009}^{2017} \phi_t * (3+kids)_{is} + \delta_2 (3+kids)_{is} + \gamma X_i + \tau_t + \theta_s + \rho_c + \xi_{its} \tag{3.6}$$

where $3+kid$ is an indicator for the child lives in a 3 or more child family. The model is restricted to children living in 2 or more child families. And all other variables are specified above. The coefficients of interest are ϕ_t. I estimate an analogous specification using the OBRA93 expansion in which the variables of interest are a child living in 2+ kid family interacted with year indicators.

Figure 3.5 plots the event study estimates for ARRA 2009 and OBRA93. In panel (a) there are no estimated differences between children living in two-child families from those living in three-child families prior to the policy change. After the 2009 policy change, children living in three child families are no more or less likely than children in two child families to report SSI income. Point estimates for years 2010 through 2012 are negative, but not statistically significant. Panel (b) shows that prior to the 1994 expansion to the EITC children in two plus child families were no more likely to report SSI income relative to one child families. After the policy in 1995, children in 2 child families were less likely to report SSI income, but this point estimate is not statistically significant. These event study analyses while noisy provide evidence that the underlying assumptions of the model hold.

3.5.2 Main Estimates

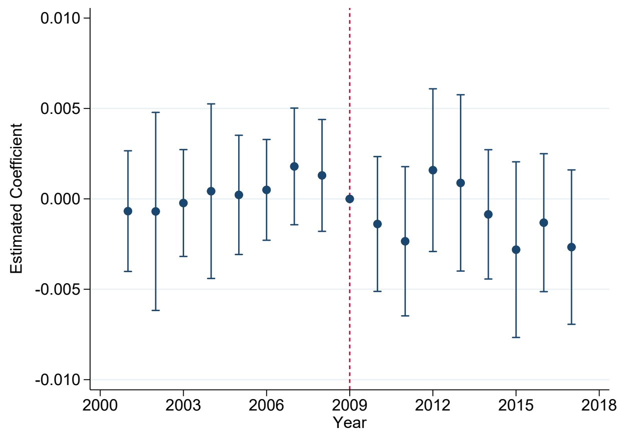
Table 3.3 presents estimates of ω_1 from equation (3.1) and β_1, β_2, and β_3 from equation (3.2). Each column represents a separate regression and results are weighted using the person ACS person weights.[10] Columns (1) and (2) present estimates utilizing variation from changes in the federal EITC, year of birth, and family composition, whereas columns (3) and (4) also include information from state EITCs. I find that exposure to an additional \$1000 each year from the federal EITC reduces the likelihood that an individual receives SSI payments by 0.45 percentage points. Given the base rate of SSI is 1.3 percentage points, this represents a 35% decline in the likelihood an individual reports income from the SSI program. When variation from state EITC credits are included, I find that exposure to an additional \$1000 each year from the EITC reduces the likelihood that a child reports income from the SSI program by 0.34 percentage points (26%).

While exposure to a larger EITC at all ages reduces disability receipt between ages 15 and 18, columns (2) and (4) both suggest that exposure to a larger EITC after age 13 has the greatest impact on reducing child disability. Focusing on column 4, I find that exposure to an additional \$1000 each year between the ages of 0 and 5 reduces the likelihood that a child receives SSI by 0.03 percentage points (but this is not statistically significant), an additional \$1000 each year between the ages of 6 and 12 reduces the likelihood that a child receives SSI by 0.10 percentage points, and that exposure to an additional \$1000 each year after age 13 reduces the likelihood that a child receives SSI by 0.25 percentage points (19%). Estimates relying solely on variation in the maximum federal EITC (column 2) are quantitatively similarly to estimates including the state variation for exposure between ages 0 and 12. However, using information solely from the federal EITC indicates that exposure to an additional \$1000 each year from the federal EITC reduces the likelihood that a child receives SSI by 0.43 percentage points (34%).

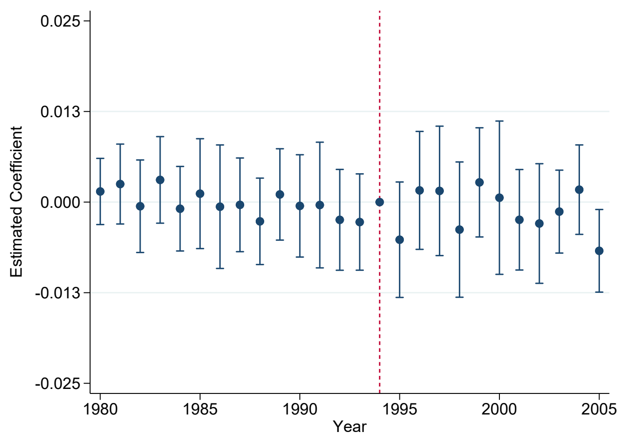
[10]Estimates are quantitatively similar when person weights are not used. See tables C.8 and C.9.

Figure 3.5: Event Time Estimates of ARRA 2009 and OBRA93 on SSI Income, Children Age 15-18

(a) ARRA 2009



(b) OBRA93



Data source: American Community Survey years 2001-2017 (panel a) and Current Population Survey years 1980-2005 (panel b).
Notes: Each figure plots coefficients and 95% confidence intervals from an event study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. two siblings versus one sibling in panel (a) or any sibling versus no siblings in panel (b)). See equation (3.6) in the text for details. The specification includes the following controls: year fixed effects, birth year fixed effects, state fixed effects, number of siblings fixed effects, child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Panel (a) shows the effect of ARRA 2009 for three child families relative to two child families. Panel (b) uses CPS data to estimate the impact of OBRA93 on families with two children relative to families with one child.

Table 3.3: Cumulative Maximum Credit Estimates of EITC on SSI Award, Children Age 15-18

	Federal EITC		State & Federal EITC	
Dependent Variable	(1)	(2)	(3)	(4)
Cumulative EITC (0-18)	-0.0045***		-0.0034***	
	(0.0004)		(0.0004)	
Cumulative EITC (0-5)		-0.0002		-0.0003
		(0.0004)		(0.0004)
Cumulative EITC (6-12)		-0.0011*		-0.0010**
		(0.0006)		(0.0004)
Cumulative EITC (13-18)		-0.0043***		-0.0025***
		(0.0007)		(0.0004)
Dep. Mean	0.0130	0.0130	0.0130	0.0130
N	1,892,085	1,892,085	1,892,085	1,892,085
State FE	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Having shown that the EITC affects SSI receipt of children, I turn to the conventional difference-in-differences strategy using the expansion in the EITC for children in 3 plus child families relative to children in 2 child families. Column 1 of Table 3.4 presents the difference-in-differences estimate, δ_1, from Equation 3.3. I find that the expansion of the EITC in 2009 reduced SSI participation by 0.04 percentage points, however, this estimate is not statistically different from zero. While the difference-in-differences estimate is not statistically significant, this result does provide suggestive evidence that the EITC may affect SSI recipiency. There are two reasons this difference-in-differences may be noisy. First, the expansion to the EITC in 2009 provided only a modest increase in EITC to families with three children versus two children, therefore, it may not have been enough money to move families away from SSI. Second, children may not immediately respond to expansions in the EITC as disability is an absorbing state.

Using the contemporaneous maximum federal and state EITC I test how exposure to a larger EITC affects contemporary SSI participation. Columns 2 and 3 of Table 3.4 present estimates of λ_1 from equation 3.4 using the maximum federal and combined maximum state and federal EITC in each year as the treatment measure. I find that exposure to an additional $1000 from the federal EITC in year t reduces the likelihood that the child claims SSI by 0.35 percentage points (27%) and exposure to an additional $1000 in state and federal EITC reduces the likelihood that a child reports income from SSI by 0.25 percentage points (19%). These estimates suggest that children are benefiting from a larger EITC and are less likely to claim SSI when they are exposed to a larger EITC.

To corroborate these findings, I supplement analyses using the Current Population Survey. Importantly, the CPS allows for a difference-in-differences analysis of the largest expansion to the EITC implemented in 1994. OBRA93 expanded the EITC for families with 2 or more qualifying children relative to families with only 1 qualifying child. Column 1 of Table 3.5 reports the difference-in-differences estimate of OBRA93 on child SSI participation. I find that OBRA reduced the likelihood that a child reports SSI income by 0.13 percentage points (16%). However, this estimate is not statistically significant. It should be noted that estimates derived from the CPS are expected to be noisier than estimates from the ACS due to sample size. The CPS, derived from a much smaller samples size, is unlikely to identify an effect on an outcome

Table 3.4: Difference-in-Differences and Maximum Credit Estimates of EITC on SSI Awards

Dependent Variable	ARRA 2009 Diff-in-Diff (1)	Contemporary Federal EITC (2)	Contemporary State & Federal EITC (3)
Treatment Coef.	-0.0004	-0.0035***	-0.0025***
	(0.0005)	(0.0004)	(0.0003)
(0.0003)	0.0120	0.0130	0.0130
N	1,503,112	2,114,608	2,114,608
State FE	YES	YES	YES
Birth Year FE	YES	YES	YES
Year FE	YES	YES	YES
Nkids FE	NO	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equations (3.3) and (3.4). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Contemporary EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

that only affects 1% of the child population. Columns 3 through 6 report point estimates from analogous regression as those reported in Table 3.3. Effects, while qualitatively similar, are not statistically different from zero. Taken together, these estimates provide suggestive evidence that the exposure to a larger EITC does reduce SSI take-up. I next turn to potential mechanisms through which a larger EITC may affect child disability.

Table 3.5: Difference-in-Differences and Cumulative Maximum Credit Estimates of EITC on SSI Awards, Current Population Survey

	OBRA 93	Federal EITC		State & Federal EITC	
Dependent Variable	(1)	(2)	(3)	(4)	(5)
Sibling*Post	-0.0013				
	(0.0012)				
Cumulative EITC (0-18)		-0.0004		-0.0004	
		(0.0007)		(0.0006)	
Cumulative EITC (0-5)			0.0016		0.0017
			(0.0014)		(0.0012)
Cumulative EITC (6-12)			-0.0025*		-0.0018
			(0.0015)		(0.0011)
Cumulative EITC (13-18)			0.0011		0.0005
			(0.0013)		(0.0010)
Dep. Mean	0.008	0.008	0.008	0.008	0.008
N	284,843	446,502	446,502	446,502	446,502
State FE	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Age x Nkids FE	NO	YES	YES	YES	YES

Date source: Current Population Survey years 1975-2017.
Notes: Each column is a separate regression. Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

3.5.3 Mechanisms

The EITC may reduce a child's participation with the SSI program through several channels. First, the EITC has been shown to improve child birth weight (Hoynes, Miller and Simon, 2015) and improve child test scores (Dahl and Lochner, 2012); therefore, the EITC may reduce the likelihood a child presents with a physical or mental disability through improved health. Second, the EITC has been shown to reduce the likelihood a child lives in poverty (Hoynes and Patel, 2014) and to increase the labor force participation of single mothers (Eissa and Liebman, 1996). Therefore, the EITC may reduce a child's eligibility for SSI through increases in family income and increases in cash-on-hand. Each of these mechanisms could be at play simultaneously. I test whether exposure to a larger EITC reduces the likelihood that a child reports a physical or mental impairment and whether the child's mother is employed.

Table 3.6 presents estimated coefficients from equations 3.1 and 3.2 using variation derived from the maximum state and federal EITC.[11] Columns (1) and (2) indicate that exposure to a larger cumulative EITC does not reduce the likelihood that a child reports a cognitive work impairment. However, I find suggestive evidence that the EITC may increase the likelihood that a family has their child screened for physical impairments. Exposure to an additional \$1000 each year between age 0 and 18 increases the likelihood that a child reports a physical work impairment by 0.12 percentage points (12%), and this appears to be driven by exposure to a larger EITC between the ages of 0 and 5. Lastly, I find no evidence that exposure to a larger EITC affects the likelihood that the child reports a visual impairment. Point estimates are small in magnitude and not statistically significant.

In Columns (7) and (8) I report estimates of equation (3.1) and (3.2) on whether a child's mother is currently in the labor force. I find no evidence that exposure to a larger EITC increases a mother's labor force participation in this sample. Exposure to an additional \$1000 each year between age 0 and 18 reduces a mother's labor force participation by 0.33 percentage points (0.48%). However, given the confidence interval, this point estimate appears to be a precise zero. The 95% confidence interval rules out an increase in labor supply greater than 0.2 percentage points (0.3%) or a reduction of labor supply greater than 0.87 percentage points (1.3%).

[11] Estimates derived solely from variation in the federal EITC are qualitatively and quantitatively similar. They are reported in the Appendix.

Table 3.6: Estimates of Cumulative Maximum State & Federal EITC on Other Outcomes, Children Age 15-18

Dep. Variable	Cognitive Impairment		Physical Impairment		Visual Impairment		Mom Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cumulative EITC (0-18)	-0.0006		0.0012**		-0.0005		-0.0033	
	(0.0008)		(0.0005)		(0.0004)		(0.0028)	
Cumulative EITC (0-5)		-0.0003		0.0007**		-0.0003		-0.0022
		(0.0007)		(0.0003)		(0.0004)		(0.0018)
Cumulative EITC (6-12)		-0.0001		0.0001		-0.0001		-0.0052
		(0.0009)		(0.0006)		(0.0007)		(0.0037)
Cumulative EITC (13-18)		0.0002		0.0003		-0.0001		0.0035
		(0.0009)		(0.0005)		(0.0005)		(0.0038)
Dep. Mean	0.0440	0.0440	0.0100	0.0100	0.0130	0.0130	0.6560	0.6560
N	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. Each health outcome is an indicator for whether the child reported a cognitive, physical, or visual impairment that impedes work. Mom Employed is an indicator that the child's mother reports employment in the survey. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Taken together, these results indicate that the EITC reduces SSI participation through channels other than improvements in child health or increases in maternal labor supply. Rather, the most likely remaining channel is that additional cash-on-hand reduces the likelihood of the family seeking additional income from disability programs. This remains an open question to be tested.

3.5.4 Heterogeneity and Subpopulation Estimates

While exposure to a larger EITC reduces the likelihood that a child participates in the SSI program, effects may vary by child race or gender. In particular, because boys make up the majority of the child SSI caseload, one may expect larger decreases in SSI participation for boys than for girls. Table 3.7 presents estimates from equations 3.1 and 3.2 for girls and boys separately. I find that exposure to an additional \$1000 each year since age 0 reduces the likelihood that a girl reports income from SSI by 0.30 percentage points (30%) while an additional \$1000 each year since age 0 reduces the likelihood that a boy reports income from SSI by 0.37 percentage points (25%). While point estimates for boys are larger, the effect sizes-given the base rate of SSI in the population-is similar. When disaggregating exposure to the EITC by age at time of exposure, I find that girls are more responsive to a larger EITC between the ages of 6 and 12. In particular, I find that an annual \$1000 increase in EITC exposure between ages 6 and 12 reduces the likelihood that a girl reports SSI income by 0.17 percentage point (17%). Similar to aggregate estimates, exposure after age 13 has the largest impact on current SSI participation. Exposure to an additional \$1000 each year since age 13 reduces the likelihood of SSI participation by 0.29 percentage points (19%) for boys and 0.21 percentage points (21%) for girls.

Columns 5-10 presents estimates disaggregated by child race. I find that non-Hispanic white children receive the largest impact from exposure to a larger EITC; an additional \$1,000 each year since age 0 reduces the likelihood that a non-Hispanic white child reports income from the SSI program by 0.35 percentage points (32%). Whereas exposure to an additional \$1000 each year since age 0 reduces the likelihood of SSI for a non-Hispanic black child by 0.43 percentage points (18%) and reduces the likelihood of SSI for an Hispanic child by 0.26 percentage points (24%).

Analysis have thus far included children in all families, regardless of parental characteristics

Table 3.7: Estimates of Cumulative Maximum State & Federal EITC on SSI Award, by Child Sex and Race

Dep. Variable	Girls		Boys		White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cumulative EITC (0-18)	-0.0030***		-0.0037***		-0.0035***		-0.0043***		-0.0026**	
	(0.0005)		(0.0006)		(0.0005)		(0.0015)		(0.0011)	
Cumulative EITC (0-5)		0.0003		-0.0008		-0.0003		0.0002		0.0009
		(0.0004)		(0.0005)		(0.0004)		(0.0012)		(0.0008)
Cumulative EITC (6-12)		-0.0017***		-0.0004		-0.0006		-0.0022		-0.0019**
		(0.0006)		(0.0007)		(0.0005)		(0.0015)		(0.0008)
Cumulative EITC (13-18)		-0.0021***		-0.0029***		-0.0031***		-0.0029		-0.0024***
		(0.0006)		(0.0007)		(0.0006)		(0.0020)		(0.0009)
Dep. Mean	0.0100	0.0100	0.0150	0.0150	0.0110	0.0110	0.0240	0.0240	0.0110	0.0110
N	911,042	911,042	981,043	981,043	1,400,419	1,400,419	220,741	220,741	315,281	315,281
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. The dependent variable is SSI receipt, or whether the child reports income from the SSI program. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

or income. Because only low-income families are eligible to receive the EITC, we may expect larger effects for children living in lower income families. However, family income may be endogenous to the EITC, therefore, I stratify the sample by the educational attainment of the mother. Children living with a mother who has completed a high school diploma or less are classified as children with a low educated mother. Table 3.8 presents estimates of equations (3.1) and (3.2) for the subsample of the population with a low-educated mother. I find that children living with a mother with a high school diploma or less are less responsive to a larger EITC. Column (3) indicates that exposure to an additional \$1000 each year since age 0 reduces the likelihood that a child receives SSI by 0.23 percentage points (14%). Again, I find that exposure to a larger EITC at later ages have the largest impact on SSI participation. I find that exposure to an additional \$1000 each year since age 13 reduces the likelihood that a child receives SSI income by 0.34 percentage points (21%). When variation is restricted to the federal EITC, I find a much larger estimate between ages 13 and 18; exposure to an additional \$1000 each year after 13 reduces the likelihood that a child reports income from SSI by 0.64 percentage points (40%).

Table 3.9 presents difference-in-differences estimates of ARRA 2009 on SSI for children with low educated mothers. I find that ARRA 2009, which expanded the EITC for families with three or more children relative to families with 2 children, decreased the likelihood that a child reported SSI income by 0.13 percentage points (8.6%) and this effect is statistically significant at the 10% level. Similar to Table 3.9, I find that exposure to a larger maximum EITC reduces concurrent SSI 0.22 percentage points (when variation induced by state EITCs is included). These estimates are consistent with the hypothesis that cash-on-hand affects a family's decision to apply for disability.

Table 3.6 reports estimates of ω_1, β_1, β_2, and β_3 from equations 3.1 and 3.2 for the subpopulation of children with low education mothers on additional outcomes. As with the full sample of all children, I find no evidence that the primary mechanism through which the EITC affects child disability is through improved child health or increased maternal labor supply. I find that for the subpopulation of children with a low educated mother, exposure to a larger cumulative EITC increases the likelihood of reporting a physical impairment and reduces the likelihood of having a working mother. Lastly, Table C.6 presents estimates for children with low education mothers disaggregated by race and gender. Results in Table C.6 are qualitatively similar to those

Table 3.8: Estimates of Cumulative Maximum EITC on SSI Award, Children with Low Educated Mother

	Federal EITC		State & Federal EITC	
Dependent Variable	(1)	(2)	(3)	(4)
Cumulative EITC (0-18)	-0.0033***		-0.0023***	
	(0.0007)		(0.0008)	
Cumulative EITC (0-5)		0.0005		0.0003
		(0.0006)		(0.0006)
Cumulative EITC (6-12)		0.0007		-0.0000
		(0.0009)		(0.0007)
Cumulative EITC (13-18)		-0.0064***		-0.0034***
		(0.0013)		(0.0010)
Dep. Mean	0.0160	0.0160	0.0160	0.0160
N	797,043	797,043	797,043	797,043
State FE	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Sample is restricted to only individuals living with a mother who has attained a high school diploma or less education. Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, and whether the child has a single mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table 3.9: Difference-in-Differences and Cumulative Maximum Credit Estimates of EITC on SSI Award, Children with Low Educated Mother

Dependent Variable	ARRA 2009 Diff-in-Diff (1)	Contemporary Federal EITC (2)	Contemporary State & Federal EITC (3)
Treatment Coef.	-0.0013*	-0.0035***	-0.0022***
	(0.0007)	(0.0007)	(0.0006)
Dep. Mean	0.0150	0.0160	0.0160
N	641,603	875,557	875,557
State FE	YES	YES	YES
Birth Year FE	YES	YES	YES
Year FE	YES	YES	YES
Nkids FE	NO	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equations (3.3) and (3.4). Sample is restricted to only individuals living with a mother who has attained a high school diploma or less education. Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, and whether the child has a single mother. Contemporary EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

presented in Table 3.7 for the full population sample.

3.5.5 Robustness

The main estimates shown previously relied on variation induced by combined federal and state EITCs. However, if the introduction of a state EITC is endogenous, meaning that they are correlated with other state level changes that are also affecting child disability, then relying on this variation may bias my estimates. Appendix tables C.2 through C.7 present analogous estimates using variation solely from the maximum federal EITC. Estimates are similar to those presented above, however, all estimates derived from the maximum federal EITC are larger in magnitude. Overall, I find that exposure to a more generous federal EITC reduces child disability, but there is no evidence that the underlying mechanism is through improved health of the child or increases in maternal labor supply.

All estimates presented thus far, were estimated using individual person weights. As a robustness, I show that my estimates are robust to unweighted specifications. Unweighted estimates represent the average treatment effect for the sampled population. Table C.8 presents estimates of equations (3.1) and (3.2). I find that exposure to an additional \$1000 each year from the state and federal EITC reduces the likelihood a child reports income from the SSI program by 0.42 percentage points (32%). Again, exposure at later ages has the largest impact on child disability. In particular, I find that exposure to an additional \$1000 each year between age 0 and age 5 reduces the likelihood a child reports income from the SSI program by 0.07 percentage points, exposure to an additional \$1000 each year between age 6 and age 12 reduces the likelihood a child reports income from the SSI program by 0.09 percentage points, and exposure to an additional \$1000 each year since age 13 reduces the likelihood a child reports income from the SSI program by 0.28 percentage points.

Table C.9 reports unweighted estimates analogous to those presented in Table 3.4. The difference-in-differences estimate of ARRA indicates that for the sampled population, ARRA 2009 reduced the likelihood that a child participates in the SSI program by 0.08 percentage points (6%) and this effect is statistically significant at the 10 percent level. Exposure to a larger contemporary maximum EITC similarly reduces the likelihood that a child participates in the SSI program.

3.6 Conclusion

This paper evaluates the interaction of two large US safety net programs for families with disabled children: the Earned Income Tax Credit and the Supplemental Security Income program for children. Using tax-reform induced variation in the federal EITC and the introduction of state EITCs, I examine the impact of the credit on child SSI program participation. I find that expansions to the EITC are associated with decreases in child disability between the ages of 15 and 18. This is true for several identification strategies: comparing children in 3 child families to children in 2 child families; comparing children in 2 child families to children in 1 child families, and using variation induced by state and federal EITC maximum credits.

Children exposed to a policy-induced \$1000 annual increase in the EITC are 26 percent less likely to participate in the SSI disability program. These impacts are evident using variation induced by both state and federal EITC or solely from federal expansions. When I examine impacts by subgroup, I find that boys and girls equally benefit from expansions to the EITC, but white children experience the largest reduction in the likelihood they participate in the SSI program.

A key limitation from this analysis is the maintained assumption that counterfactual disability outcomes within state and across family size would be constant over time. However, event study analysis show that preexisting trends are flat (they also show no effect in the post period) for both comparison groups (three child families to two child families in ACS, and two child families to one child families in the CPS). This provides reassurance that the underlying assumptions of the model are met and I am estimating the causal impact of the EITC expansions.

My results suggest that there are non-trivial benefits of the EITC on child disability. However, I find no evidence that the mechanism through which the EITC reduces child disability is through improved child health. I find no reduction in the likelihood a child reports a cognitive work impairment; and I find evidence that expansions of the EITC increased the likelihood a child reports a physical work impairment. I find no evidence that the mechanism through which the EITC reduces child disability is mechanical--through increases in maternal labor supply and subsequent increases in pre-tax income. A remaining hypothesis posits that additional "cash-on-hand" from the EITC reduces the likelihood that a family seeks disability income. This is supported by the finding that exposure to a larger contemporary EITC and exposure to a larger annual cumulative EITC between the ages of 13 and 18 have the largest impacts on child disability. This hypothesis remains an open question to be examined.

This remains an active project. Important to note are the following: first, the change in SSI reporting in 2010 may be biasing results reported using the ACS data, and second, *Zebley v. Sullivan* in 1990 and Welfare Reform in 1996 may bias estimates of OBRA93. Therefore, as future steps on this project I am exploring use of additional data sources including the Synthetic SIPP data. In sum, the impact of the Earned Income Tax Credit on child disability remains an open question for future work.

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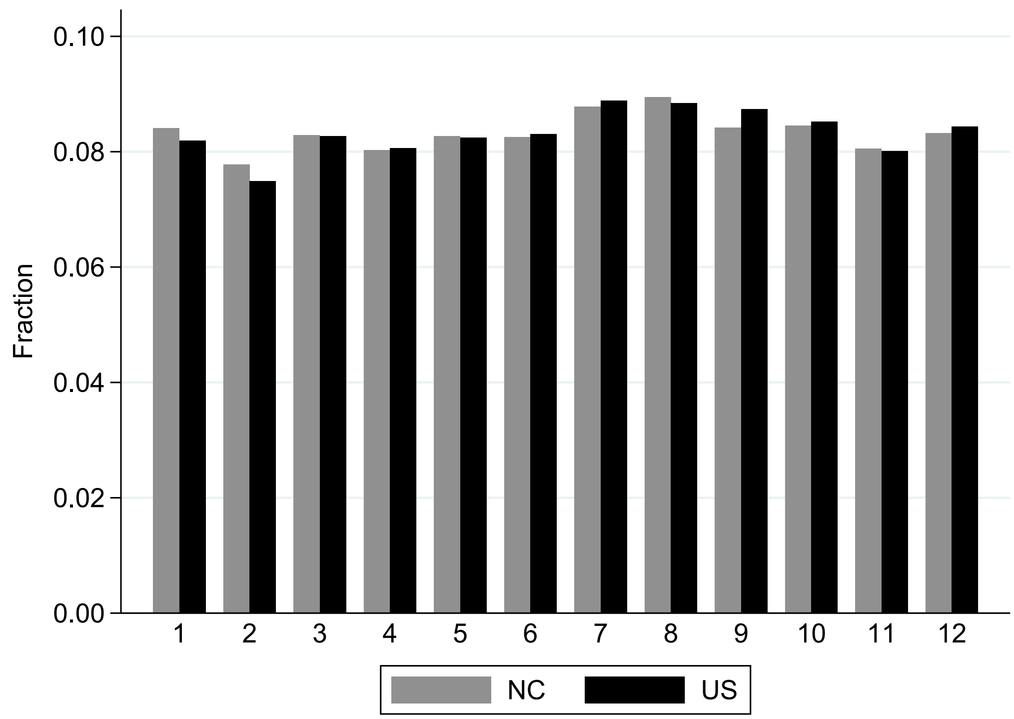
Appendix

A Chapter 1 Appendix

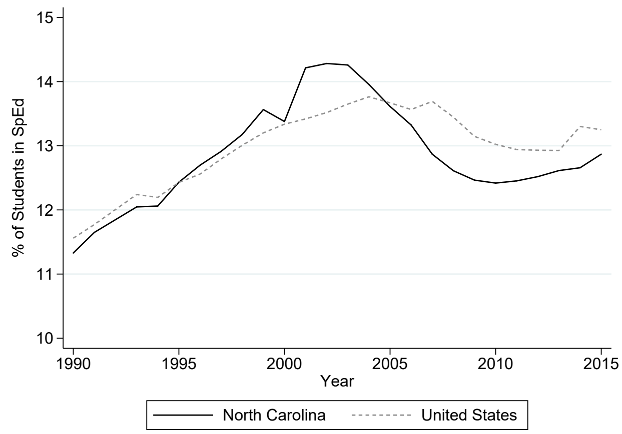
A.1 Figures

Figure A.1: Distribution of Births by Month of Birth, US Total and NC Public School Data

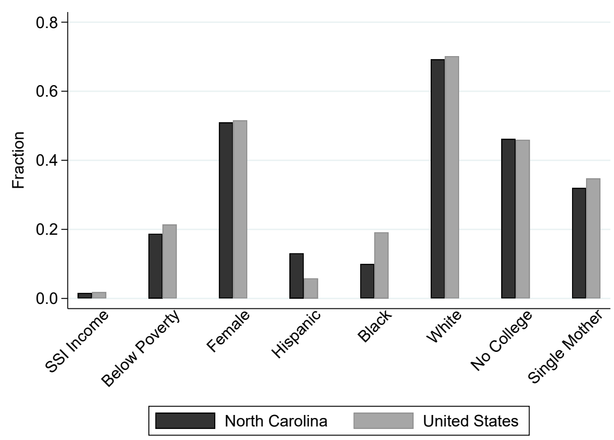


Data source: North Carolina Department of Public Instruction, school years 2004-2014 and National Vital Statistics Reports, Vol 66. No. 1 January, 2017. Notes: US includes North Carolina.

Figure A.2: Characteristics of North Carolina Compared to US Average
(a) Special Education in North Carolina and United States



(b) Characteristics of Children in North Carolina and United States



Data source: National Center for Education Statistics Digest of Education Statistics 1995-2017 (panel a) and American Community Survey, 2002-2015 (panel b). Notes: United States does not include North Carolina. No College is an indicator for the child lives with a mother with a high school diploma or less. Single Mother is an indicator for a child living with a single mother.

A.2 Tables

Table A.1: Regression Discontinuity Estimates of Being Youngest in Classroom on Grade 3 Outcomes Various Model Specifications

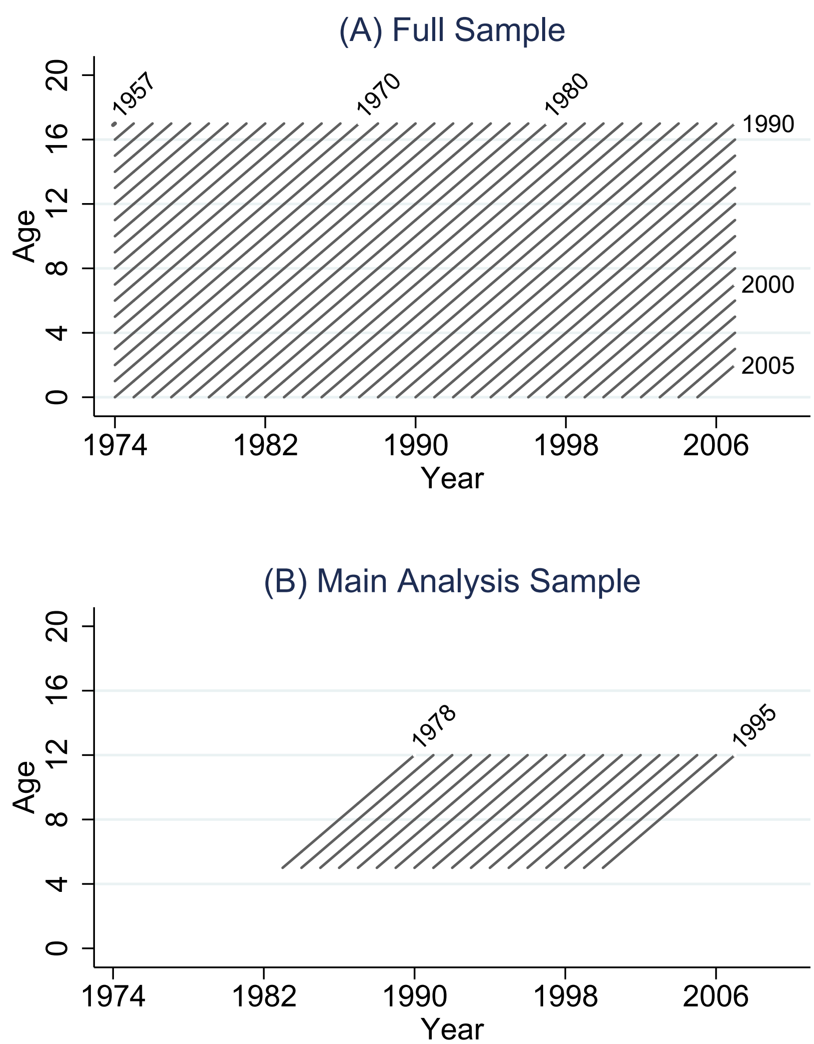
	Age at Grade 3 Exam (1)	Grade 3 Math Z-Score (2)	Grade 3 Read Z-Score (3)	Receives SpEd in Grade 3 (4)	Repeated Grade 3 (5)
Local Linear	-0.5952***	-0.1009***	-0.1244***	0.0175***	0.0116***
	(0.0030)	(0.0041)	(0.0041)	(0.0015)	(0.0006)
Local Quadratic	-0.4495***	-0.1128***	-0.1388***	0.0205***	0.0081***
	(0.0046)	(0.0077)	(0.0078)	(0.0029)	(0.0012)
Local Cubic	-0.4021***	-0.01154***	-0.1415***	0.0332***	0.0025
	(0.0085)	(0.0168)	(0.0166)	(0.0061)	(0.0026)
Local Quartic	-0.5530***	-0.0259	-0.0279	0.0556***	-0.0077
	(0.0231)	(0.0484)	(0.0488)	(0.0183)	(0.0079)
Dep. Mean	9.4511	0.0596	0.0638	0.1062	0.0158
N	1,083,943	1,075,424	1,074,108	1,083,946	1,083,946

Data source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Sample includes all children who attended a North Carolina public school for grade 3. Each cell represents a separate regression. Results reflect estimates of equation (1.2). All models include fixed effects for birth year and school. Standard errors are clustered at the school and are reported in parentheses. Special education includes thirteen separate classifications of disabilities. Each test has been normalized within year, grade, and test to have a mean zero and standard deviation of one. Repeated grade is an indicator that the child was recorded in third grade in two consecutive years. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

B Chapter 2 Appendix

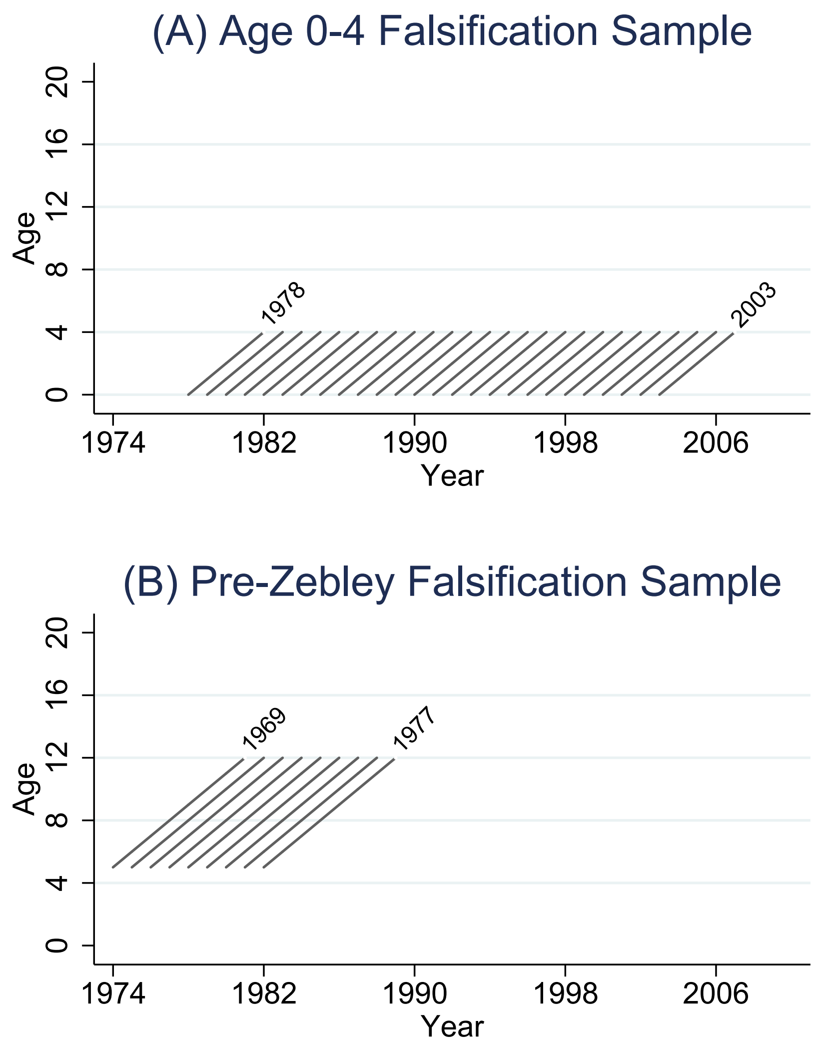
B.1 Figures

Figure B.1: Lexus Diagram of Sample



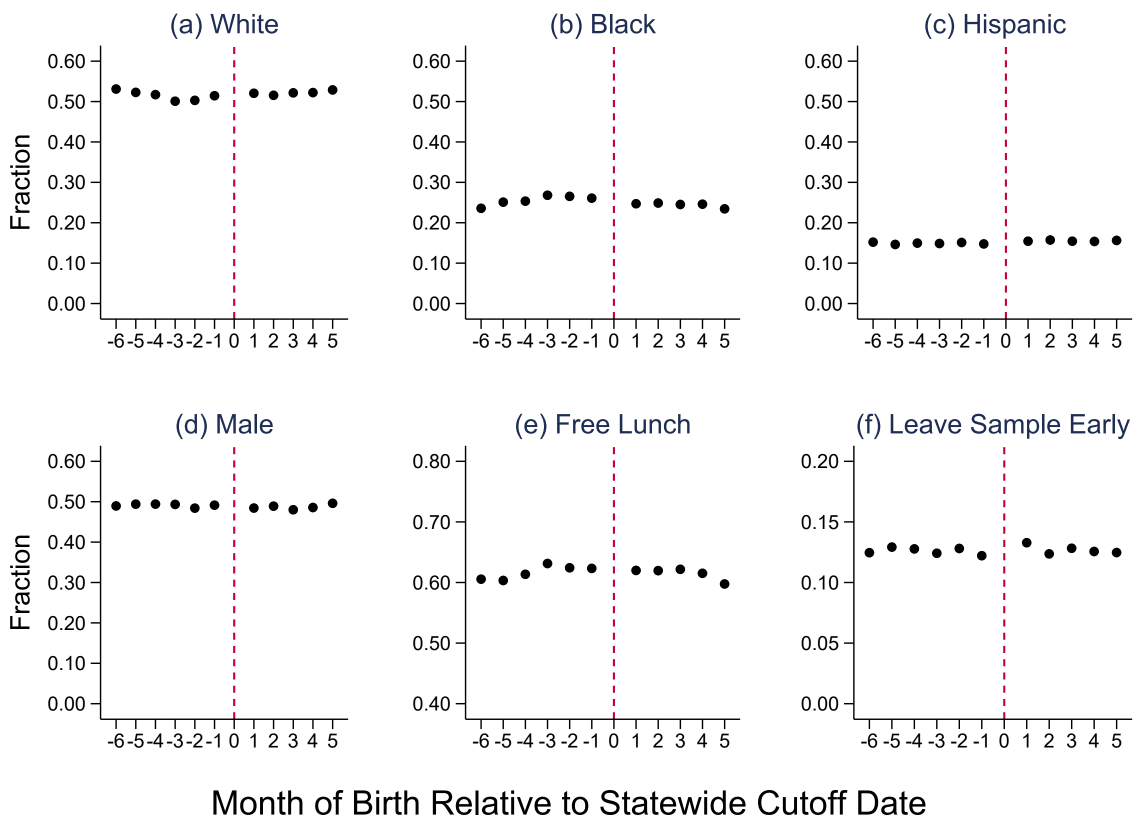
Notes: The data utilized comes from NHIS Survey years 1994-2005. Respondents in these surveys have been linked to the Supplemental Security Record (SSR) of all SSI applications and awards between 1974 and 2007. For each NHIS survey respondent in survey years 1994-2005, I construct indicators for whether that individual applied for and/or received an award between the ages of 0 and 17. Thus, the sample includes individuals born between 1957 and 2005. Each line in the graph represents a birth cohort. Looking across age horizontally will reflect the potential birth cohorts included in the analysis.The main analysis considers disability outcomes that occurred between the ages of 5 and 12 for individuals who experienced *Zebley* SSI rules. Therefore, the main sample includes cohorts who turn 12 between 1990 and 2007. This is represented by birth cohorts 1978-1995.

Figure B.2: Lexus Diagram of Falsification Samples



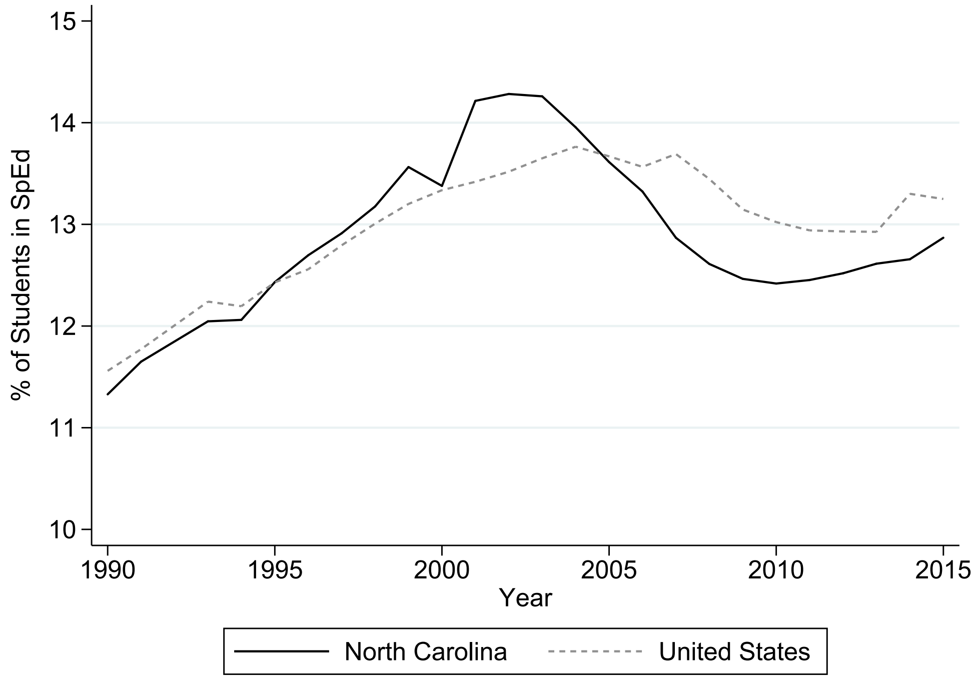
Notes: I utilize two falsification samples. Panel (A) represents the 0 to 4 falsification sample, which includes birth cohorts 1978-2003. The individuals in this sample should not be affected by a school entry cutoff date, since the disability claims during this age range are prior to school entry. Panel (B) shows the pre-Zebley sample which considers disability outcomes that occurred between the ages of 5 and 12, for individuals who turn 12 before 1989, that is birth cohorts 1969-1977. These individuals in this sample should not be affected by the school entry cutoff date because there should be no interaction between special education and SSI for children.

Figure B.3: Count of Births by Relative Age by Observable Characteristics, North Carolina Sample



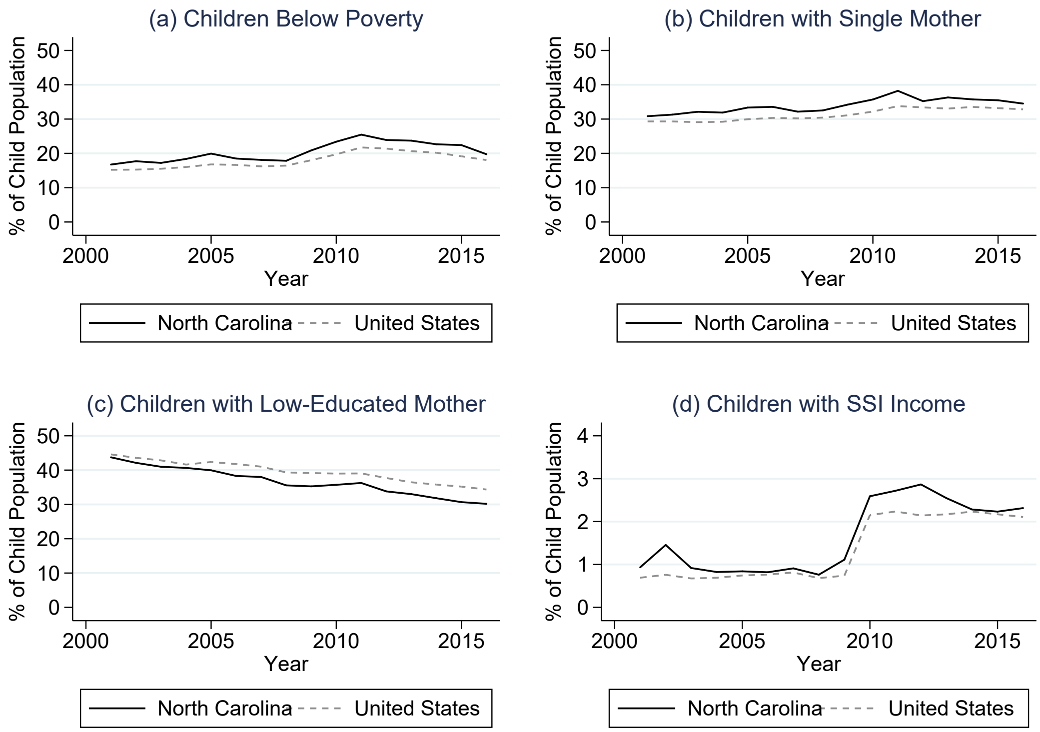
Data Source: North Carolina Department of Public Instruction, school years 2004-2014.
Notes: Includes children in kindergarten cohorts 2006, 2007, and 2008. All October births are dropped from the sample.

Figure B.4: Fraction of Children Receiving Special Education Services in North Carolina and US 1990-2015



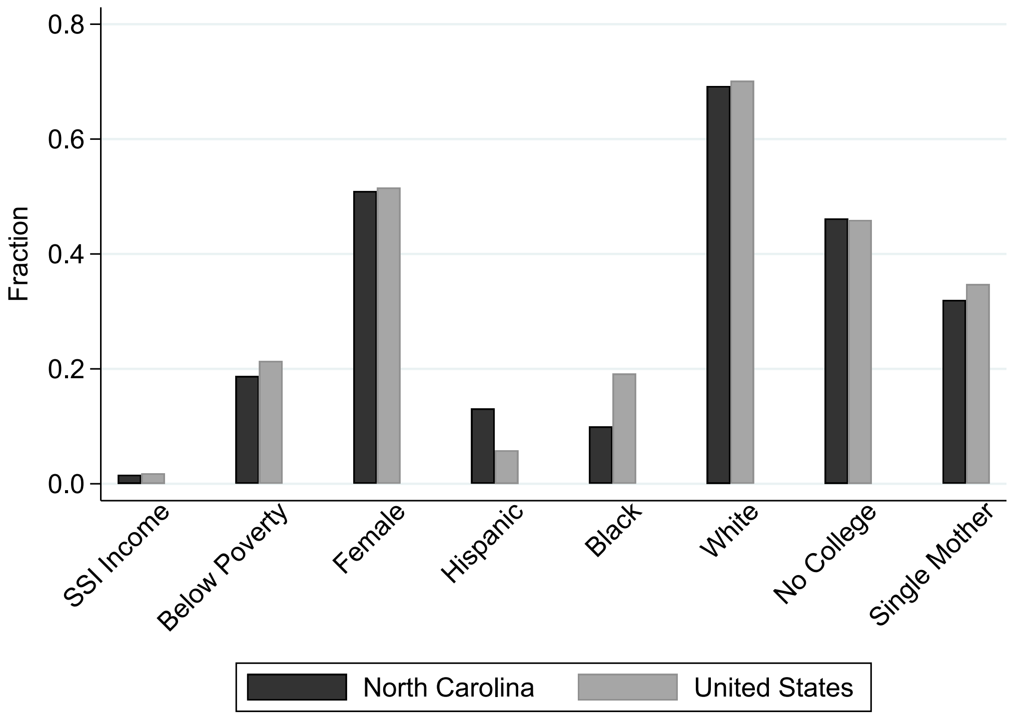
Data Source: National Center for Education Statistics Digest of Education Statistics 1995-2017.

Figure B.5: Characteristics of Children in North Carolina and US 2000-2015



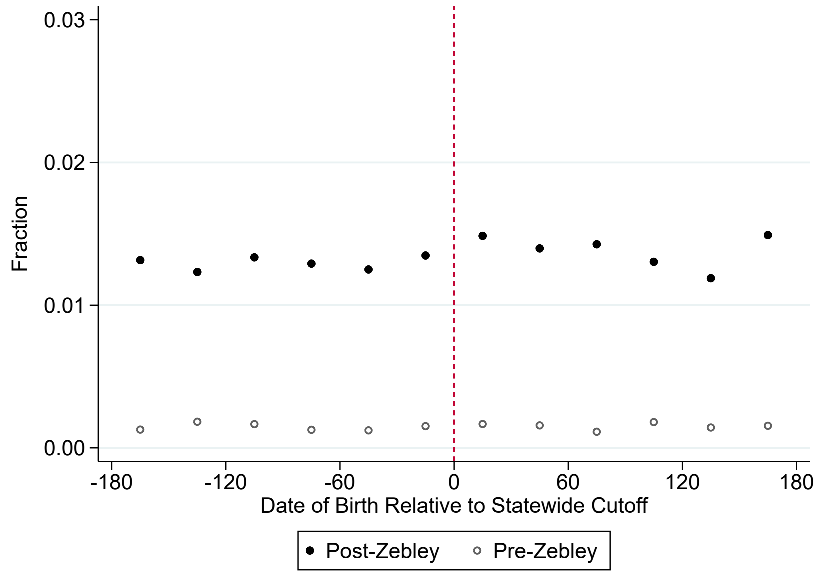
Data Source: American Community Survey, 2002-2015. North Carolina tends to trend similarly to the US (without North Carolina).

Figure B.6: Characteristics of Children in North Carolina and US Averaged between 2000-2015



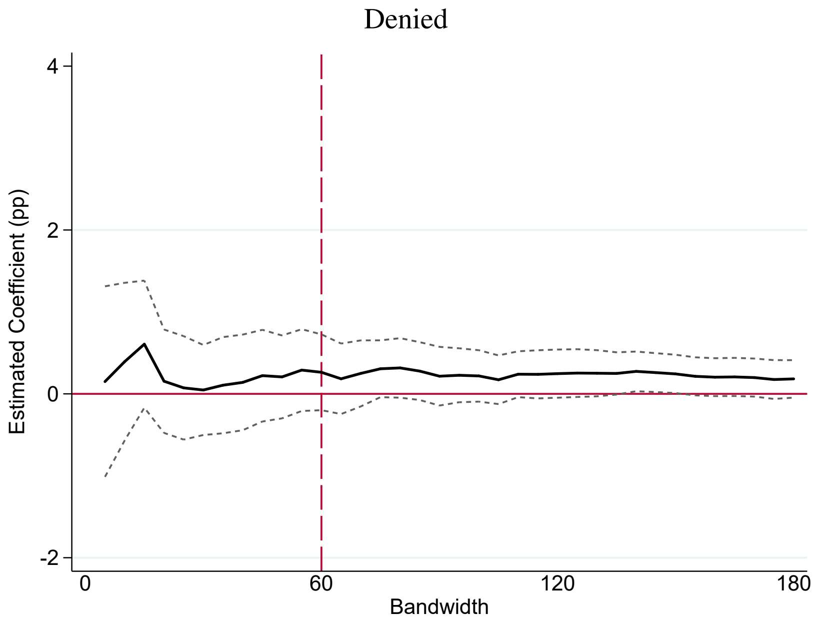
Data Source: American Community Survey, 2002-2015. No college and Single Mother indicate the fraction of children with a parent with a high school education or less or a single mother. Overall, North Carolina has a larger Hispanic population and a smaller black population than the combined US without NC.

Figure B.7: Denied Applications for SSI by Relative Age, Pre-Zebley and Post-Zebley Cohorts



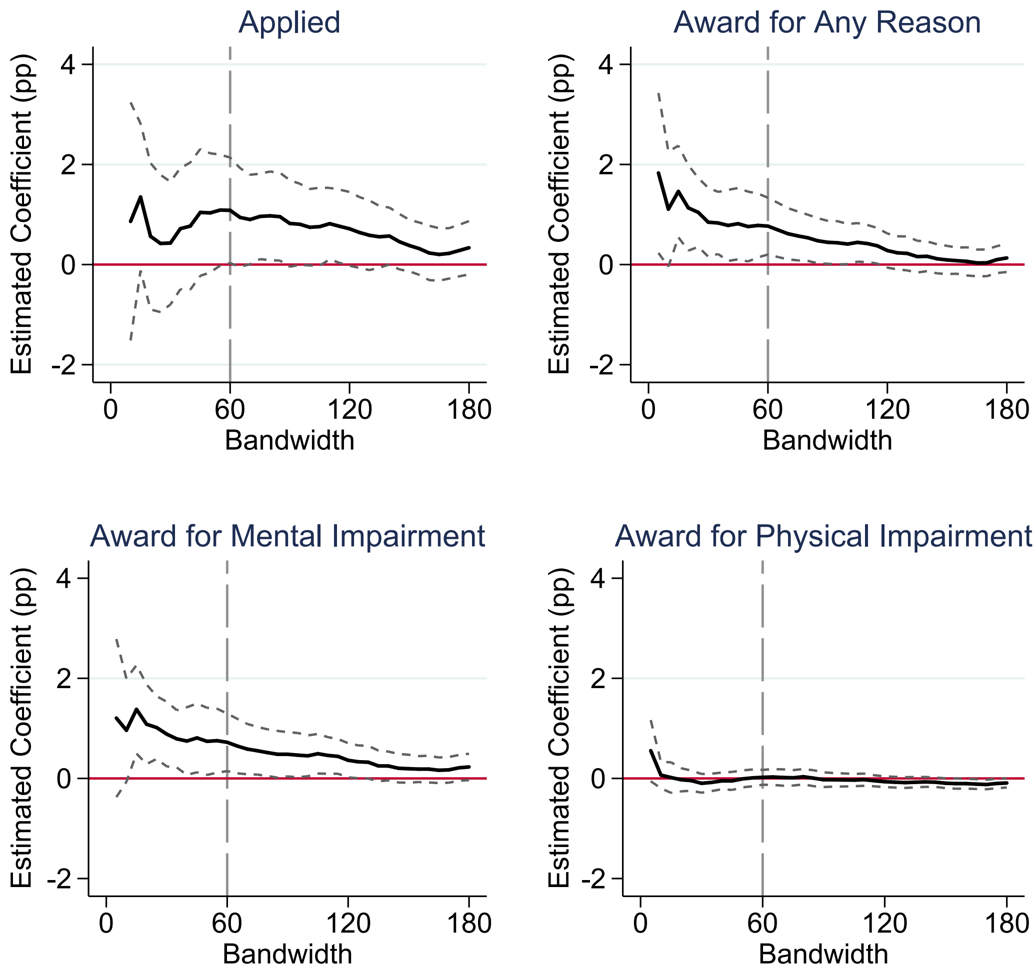
Data Source: NHIS Surveys 1994-2005, birth cohorts 1957-1995.
Notes: The above graphs plot the fraction of individuals who have been denied SSI between the ages of 5 and 12 for the post-*Zebley* and pre-*Zebley* cohorts.The data have been aggregated to 30 day bins. Dates to the left of the cutoff represent individuals born after the statewide cutoff date (or the Oldest in the classroom), while dates to the right of the cutoff represent individuals born before the statewide cutoff date (or the Youngest in the classroom).

Figure B.8: Robustness of Estimates to Bandwidth



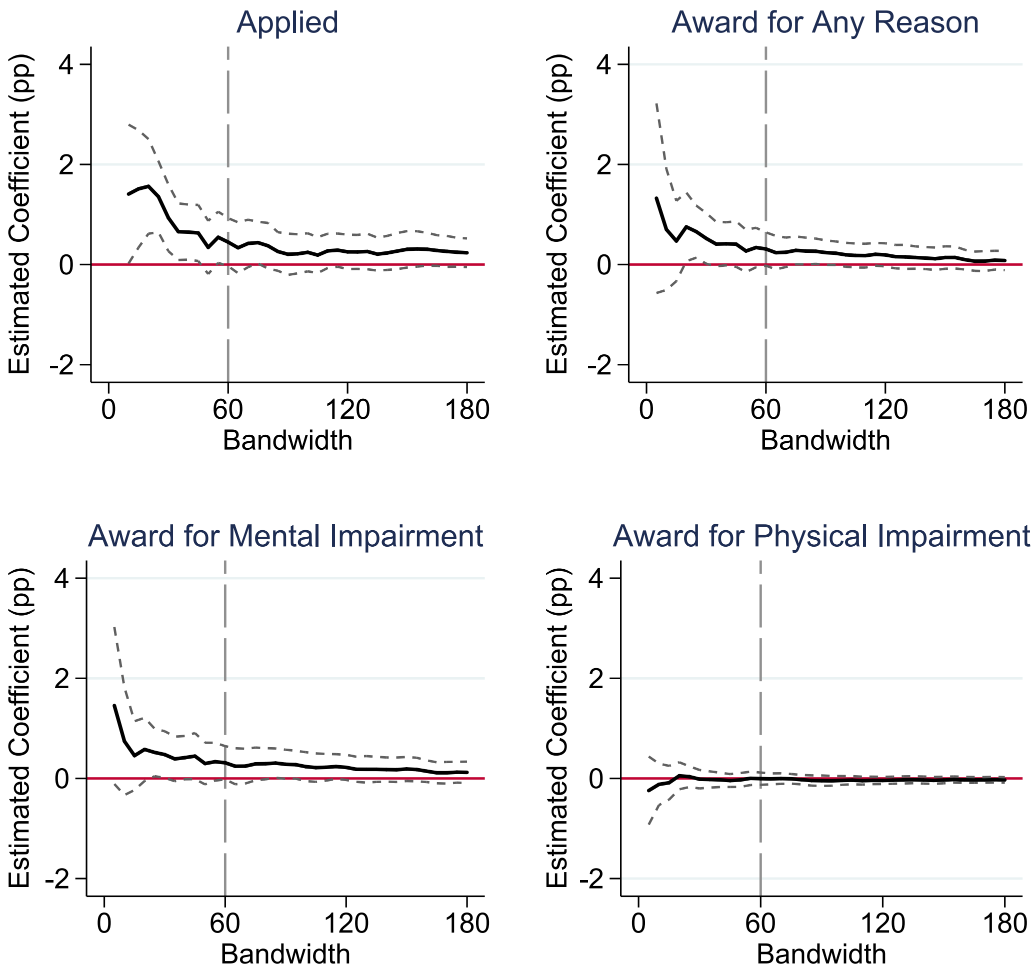
Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths ranging from 5 to 180 days. The local linear regressions are estimated using robust standard errors clustered at the state.

Figure B.9: Robustness of Estimates to Bandwidth, Boys



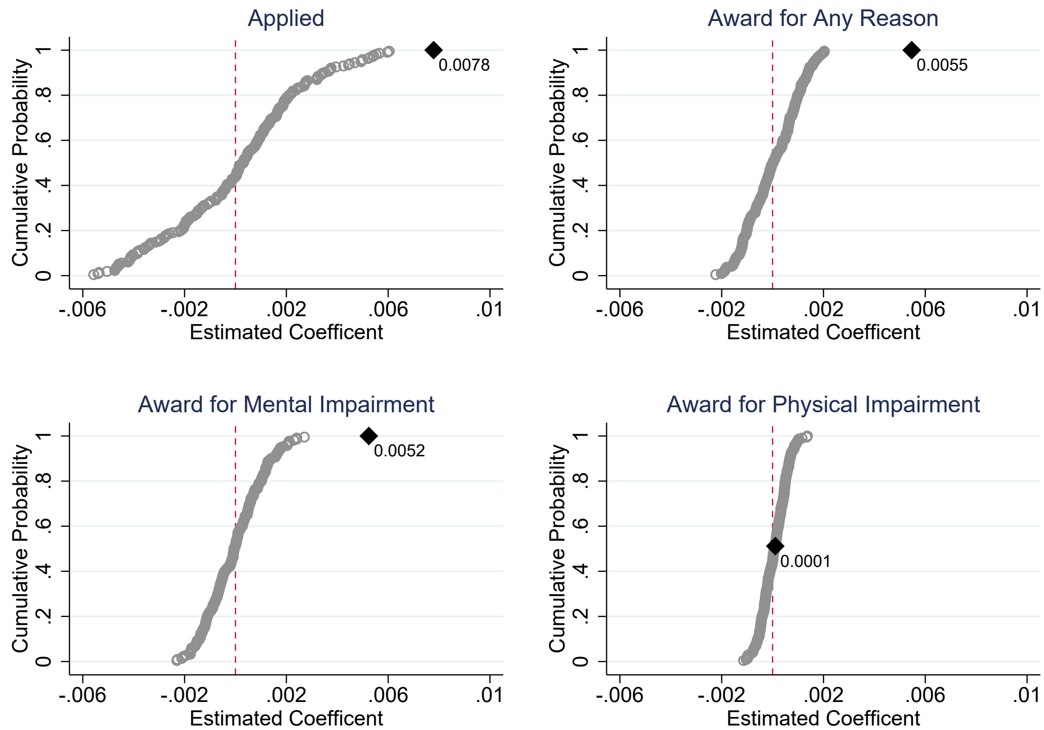
Data Source: NHIS Surveys 1994-2005, males born in birth cohorts 1978-1995.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths ranging from 5 to 180 days. The local linear regressions are estimated using robust standard errors clustered at the state.

Figure B.10: Robustness of Estimates to Bandwidth, Girls



Data Source: NHIS Surveys 1994-2005, females born in birth cohorts 1978-1995.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals at different bandwidths ranging from 5 to 180 days. The local linear regressions are estimated using robust standard errors clustered at the state.

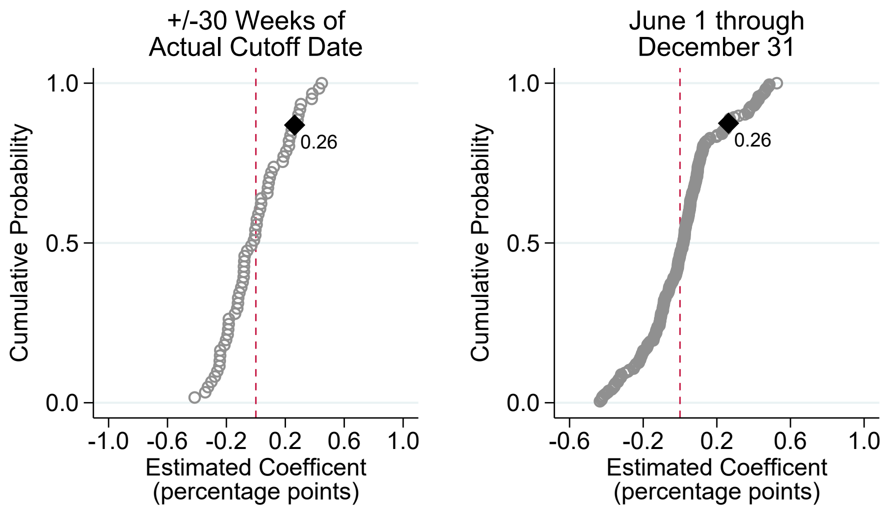
Figure B.11: The Distribution of Disability Estimates for Placebo Cutoff Dates June 1 through December 31



Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.

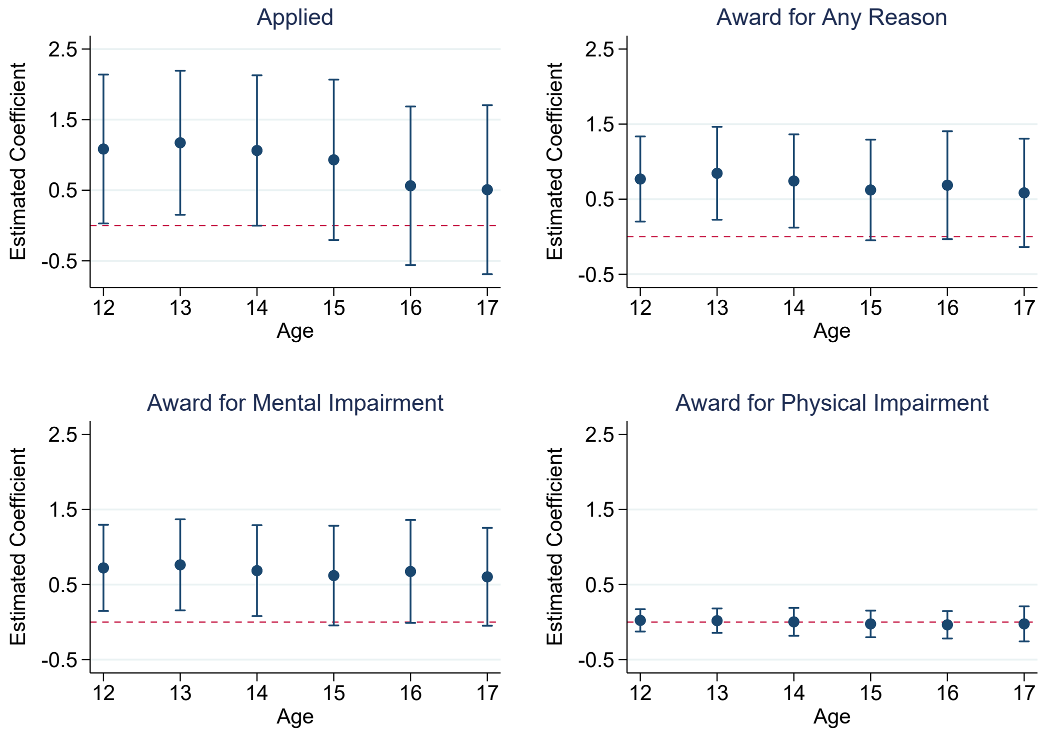
Notes: These figures show the conditional density functions of point estimates using each day between June 1 and December 31 as placebos. The diamond represents the regression discontinuity estimated at the true state cutoff date. All models are specified as a local linear regression with a bandwidth of 60 days.

Figure B.12: The Distribution of Disability Estimates for Denied Applications



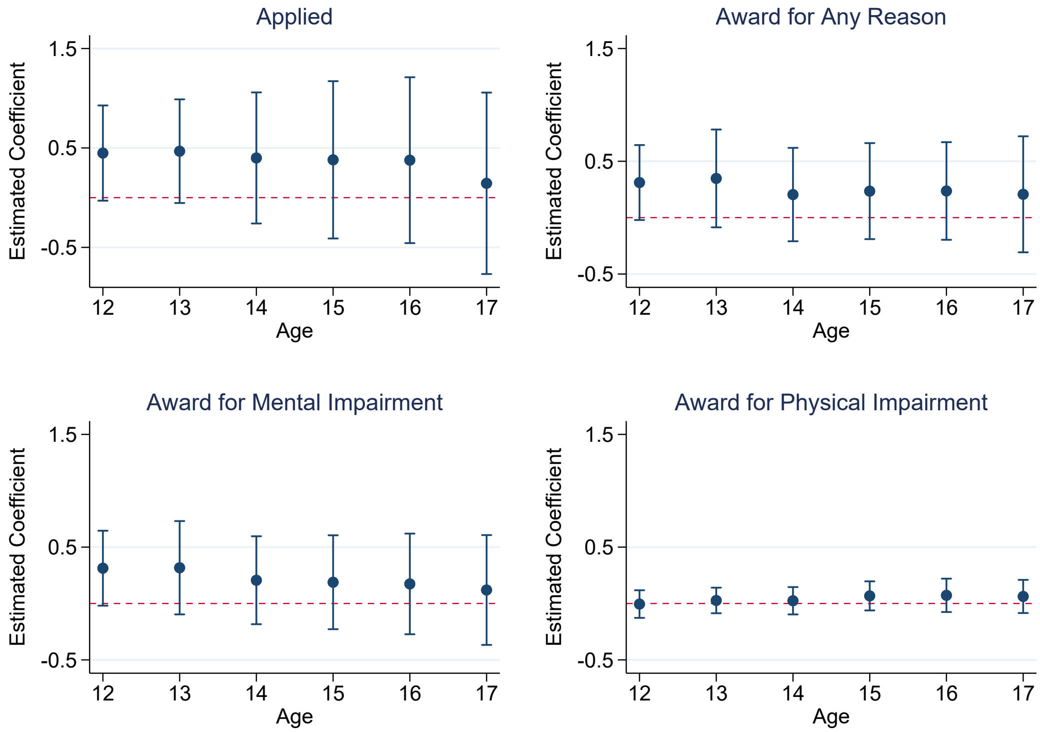
Data Source: NHIS Surveys 1994-2005, birth cohorts 1978-1995.
Notes: These figures show the conditional density functions of point estimates using each day between June 1 and December 31 as placebos. The diamond represents the regression discontinuity estimated at the true state cutoff date. All models are specified as a local linear regression with a bandwidth of 60 days.

Figure B.13: Dissipation of RD Estimates for Ages 13 to 17, Boys



Data Source: NHIS Surveys 1994-2005, Boys born in birth cohorts which vary with age.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals for applications and awards between ages 12 and 17. Each age represents the likelihood of applying or receiving an award between age 5 and the given age. The local linear regressions are estimated using robust standard errors clustered at the state with a bandwidth of 60 days. The effect of the statewide school cutoff date dissipates after age 13. Reasons for why these effects dissipate between ages 13 and 17 are discussed in the text.

Figure B.14: Dissipation of RD Estimates for Ages 13 to 17, Girls



Data Source: NHIS Surveys 1994-2005, Girls born in birth cohorts which vary with age.
Notes: These figures show the coefficient estimates and 95 percent confidence intervals for applications and awards between ages 12 and 17. Each age represents the likelihood of applying or receiving an award between age 5 and the given age. The local linear regressions are estimated using robust standard errors clustered at the state with a bandwidth of 60 days. The effect of the statewide school cutoff date dissipates after age 13. Reasons for why these effects dissipate between ages 13 and 17 are discussed in the text.

B.2 Tables

Table B.1: Regression Discontinuity Estimates on Observable Characteristics

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
White	1.06	1.07	1.01
	(0.76)	(1.02)	(0.78)
Dep. Mean	46.89	47.10	46.67
Black	-0.43	-0.44	-0.41
	(0.58)	(0.61)	(0.81)
Dep. Mean	15.23	15.12	15.34
Hispanic	-1.18	-1.61*	-0.69
	(0.91)	(0.95)	(1.00)
Dep. Mean	32.55	32.61	32.48
Male	1.33*		
	(0.72)		
Dep. Mean	50.58		
N	71,089	36,119	34,970

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. Local linear regressions are specified with a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Estimates on the race and sex controls variables show that these characteristics are smooth through the cutoff date.

Table B.2: Regression Discontinuity Estimates of the Increase in Disability by Type of Mental Impairment at Statewide Entry Cutoff Date

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
Award for Mental Impairment	0.0052***	0.0072**	0.0031*
	(0.0018)	(0.0029)	(0.0017)
Dep. Mean	0.0076	0.0107	0.0044
Award for Malleable Mental Impairment	0.0035***	0.0045**	0.0023**
	(0.0011)	(0.0020)	(0.0010)
Dep. Mean	0.0029	0.0049	0.0009
Award for Mental Retardation	0.0017	0.0021	0.0013
	(0.0010)	(0.0016)	(0.0013)
Dep. Mean	0.0039	0.0047	0.0032

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. Local linear regressions are specified with a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. "Malleable" mental awards include: affective disorders, anxiety disorders, personality disorders, conduct disorders, defiant disorders, ADHD, speech impairments, and learning disabilities. Mental retardation is composed of two classifications: mental retardation and borderline mental retardation. Omitted mental impairments that are not included as malleable impairments are as follows: organic mental disorder, schizophrenic disorders, autism, and substance abuse. Malleable mental impairments account for the majority of the increase in all awards at the cutoff date.

Table B.3: Regression Discontinuity Estimates of the Increase in Disability at Statewide Entry Cutoff Date, By Child Sex

Dependent Variable	Boys (1)	Girls (2)
Applied	0.0108**	0.0045*
	(0.0054)	(0.0024)
Dep. Mean	0.0331	0.0185
Award for Any Reason	0.0077***	0.0031*
	(0.0029)	(0.0017)
Dep. Mean	0.0129	0.0057
Award for Mental Impairment	0.0072**	0.0031*
	(0.0029)	(0.0017)
Dep. Mean	0.0107	0.0044
Award for Physical Impairment	0.0002	-0.0000
	(0.0008)	(0.0006)
Dep. Mean	0.0017	0.0010
Denied	0.0026	0.0026
	(0.0041)	(0.0020)
Dep. Mean	0.0194	0.0123
N	28,952	28,407

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1969-1995. Each cell represents a separate regression. Local linear regressions are specified with a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12.

Table B.4: Robustness of RD Estimates to Local Polynomial Choice

Dependent Variable	Linear (1)	Quadratic (2)	Cubic (3)	Quartic (4)
Applied	0.0078***	0.0076**	0.0109**	0.0158**
	(0.0027)	(0.0038)	(0.0050)	(0.0071)
Award for Any Reason	0.0055***	0.0070***	0.0100***	0.0122**
	(0.0016)	(0.0027)	(0.0028)	(0.0048)
Award for Mental Impairment	0.0052***	0.0071**	0.0090***	0.0108**
	(0.0018)	(0.0027)	(0.0028)	(0.0044)
Award for Physical Impairment	0.0001	-0.0007	-0.0002	0.0006
	(0.0006)	(0.0007)	(0.0010)	(0.0012)
Denied	0.0026	0.0008	0.0013	0.0044
	(0.0024)	(0.0034)	(0.0038)	(0.0049)
N	57,359			

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12.

Table B.5: Robustness of RD Estimates to Local Polynomial Choice by Child Sex

Dependent Variable	Linear (1)	Quadratic (2)	Cubic (3)	Quartic (4)
Boys				
Applied	0.0108**	0.0062	0.0041	0.0107
	(0.0054)	(0.0070)	(0.0080)	(0.0105)
Award for Any Reason	0.0077***	0.0092**	0.0119***	0.0169***
	(0.0029)	(0.0041)	(0.0045)	(0.0061)
Award for Mental Impairment	0.0072**	0.0095**	0.0114***	0.0157***
	(0.0029)	(0.0040)	(0.0037)	(0.0056)
Award for Physical Impairment	0.0002	-0.0010	-0.0005	0.0014
	(0.0008)	(0.0011)	(0.0012)	(0.0020)
Denied	0.0026	-0.0033	-0.0072	-0.0050
	(0.0041)	(0.0056)	(0.0059)	(0.0087)
N	28,952			
Girls				
Applied	0.0045*	0.0089**	0.0172***	0.0200**
	(0.0024)	(0.0036)	(0.0055)	(0.0080)
Award for Any Reason	0.0031*	0.0049	0.0081**	0.0073
	(0.0017)	(0.0031)	(0.0039)	(0.0060)
Award for Mental Impairment	0.0031*	0.0048*	0.0066*	0.0060
	(0.0017)	(0.0028)	(0.0037)	(0.0053)
Award for Physical Impairment	-0.0000	-0.0004	-0.0000	-0.0003
	(0.0006)	(0.0009)	(0.0019)	(0.0022)
Denied	0.0026	0.0048*	0.0094**	0.0130***
	(0.0020)	(0.0028)	(0.0041)	(0.0048)
N	28,407			

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12.

Table B.6: Robustness of RD Estimates to Sample Weights

Dependent Variable	Unweighted (1)	NHIS Person Weight (2)	Weight Adjusted for Link-Eligibility (3)
Applied	0.0078***	0.0094***	0.0082***
	(0.0027)	(0.0029)	(0.0031)
Award for Any Reason	0.0055***	0.0058***	0.0059***
	(0.0016)	(0.0017)	(0.0018)
Award for Mental Impairment	0.0052***	0.0054***	0.0054***
	(0.0018)	(0.0017)	(0.0020)
Award for Physical Impairment	0.0001	0.0002	0.0002
	(0.0006)	(0.0007)	(0.0007)
Denied	0.0026	0.0041*	0.0028
	(0.0024)	(0.0024)	(0.0022)
N	57,359	57,260	57,260

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12. See text for details on the weights.

Table B.7: Robustness of RD Estimates to Sample Weights by Child Sex

Dependent Variable	Unweighted (1)	NHIS Person Weight (2)	Weight Adjusted for Link-Eligibility (3)
Boys			
Applied	0.0108**	0.0125**	0.0105*
	(0.0054)	(0.0053)	(0.0058)
Award for Any Reason	0.0077***	0.0070**	0.0069**
	(0.0029)	(0.0031)	(0.0035)
Award for Mental Impairment	0.0072**	0.0065**	0.0063*
	(0.0029)	(0.0029)	(0.0033)
Award for Physical Impairment	0.0002	0.0002	0.0005
	(0.0008)	(0.0010)	(0.0011)
Denied	0.0026	0.0050	0.0030
	(0.0041)	(0.0041)	(0.0039)
N	28,952		
Girls			
Applied	0.0045*	0.0059**	0.0057**
	(0.0024)	(0.0027)	(0.0027)
Award for Any Reason	0.0031*	0.0043**	0.0047***
	(0.0017)	(0.0019)	(0.0017)
Award for Mental Impairment	0.0031*	0.0040**	0.0043**
	(0.0017)	(0.0018)	(0.0017)
Award for Physical Impairment	-0.0000	0.0002	-0.0000
	(0.0006)	(0.0008)	(0.0007)
Denied	0.0026	0.0030	0.0026
	(0.0020)	(0.0024)	(0.0024)
N	28,407		

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12. See text for details on the weights.

Table B.8: Robustness of RD Estimates to Assumptions About the Standard Errors

Dependent Variable	State (1)	Not Clustered (2)	Birth Year (3)	Relative Age (4)
Applied	0.0078***	0.0078***	0.0078**	0.0078***
	(0.0027)	(0.0027)	(0.0032)	(0.0025)
Award for Any Reason	0.0055***	0.0055***	0.0055***	0.0055***
	(0.0016)	(0.0016)	(0.0016)	(0.0015)
Award for Mental Impairment	0.0052***	0.0052***	0.0052***	0.0052***
	(0.0018)	(0.0015)	(0.0015)	(0.0013)
Award for Physical Impairment	0.0001	0.0001	0.0001	0.0001
	(0.0006)	(0.0006)	(0.0003)	(0.0006)
Denied	0.0026	0.0026	0.0026	0.0026
	(0.0024)	(0.0021)	(0.0025)	(0.0019)
N	57,359			

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12. See text for details about the various levels at which the estimates are clustered.

Table B.9: Robustness of RD Estimates to Assumptions of Standard Errors, by Child Sex

Dependent Variable	State (1)	Not Clustered (2)	Birth Year (3)	Relative Age (4)
Boys				
Applied	0.0108**	0.0108**	0.0108**	0.0108***
	(0.0054)	(0.0042)	(0.0043)	(0.0038)
Award for Any Reason	0.0077***	0.0077***	0.0077***	0.0077***
	(0.0029)	(0.0027)	(0.0020)	(0.0024)
Award for Mental Impairment	0.0072**	0.0072***	0.0072***	0.0072***
	(0.0029)	(0.0025)	(0.0021)	(0.0021)
Award for Physical Impairment	0.0002	0.0002	0.0002	0.0002
	(0.0008)	(0.0009)	(0.0007)	(0.0010)
Denied	0.0026	0.0026	0.0026	0.0026
	(0.0041)	(0.0033)	(0.0037)	(0.0033)
N	28,952			
Girls				
Applied	0.0045*	0.0045	0.0045	0.0045
	(0.0024)	(0.0032)	(0.0037)	(0.0035)
Award for Any Reason	0.0031*	0.0031*	0.0031**	0.0031*
	(0.0017)	(0.0018)	(0.0015)	(0.0019)
Award for Mental Impairment	0.0031*	0.0031*	0.0031**	0.0031*
	(0.0017)	(0.0017)	(0.0015)	(0.0016)
Award for Physical Impairment	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0006)	(0.0007)	(0.0005)	(0.0007)
Denied	0.0026	0.0026	0.0026	0.0026
	(0.0020)	(0.0027)	(0.0034)	(0.0029)
N	28,407			

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data is from restricted-use NHIS-SSA linked data and include birth cohorts 1978-1995. Each cell represents a separate regression. All specifications use a bandwidth of 60 days. Robust standard errors are reported in parenthesis and are clustered at the state. The mean of the individuals to the left of the cutoff (the oldest in the classroom) is reported. Outcomes are defined as indicators for the SSI event occurring between the ages of 5 and 12. See text for details about the various levels at which the estimates are clustered.

Table B.10: RD Estimates of the Increase in Special Education Services at the School Cutoff Date, Sample Includes Early Leavers

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
Special Education	0.0300***	0.0362***	0.0249***
	(0.0034)	(0.0054)	(0.0043)
Dep. Mean	0.11	0.15	0.09
N	259,787	127,439	132,348

Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. Data come from the North Carolina Department of Public Instruction, school years 2004-2014. The outcome of interest is receipt of special education services between the ages of 5 and 12 (grade K-grade 6). All October births are dropped due to the October 16 cutoff date. This sample is not restricted to children who remained in the NC public school system for all years between grade 3 and grade 6, therefore, it includes children who may have been enrolled in special education had they not left the system.

Table B.11: Two-sample Fuzzy RD Estimates of Special Education on SSI, Sample Includes Early Leavers

Dependent Variable	Boys and Girls (1)	Boys (2)	Girls (3)
Award for Mental Impairment	0.2567***	0.2983*	0.1767*
	(0.0945)	(0.1529)	(0.1010)
Dep. Mean	0.0259	0.0331	0.0185
Award for Malleable Mental Impairment	0.1800***	0.2099**	0.1245*
	(0.0570)	(0.0833)	(0.0643)
Dep. Mean	0.0093	0.0129	0.0057

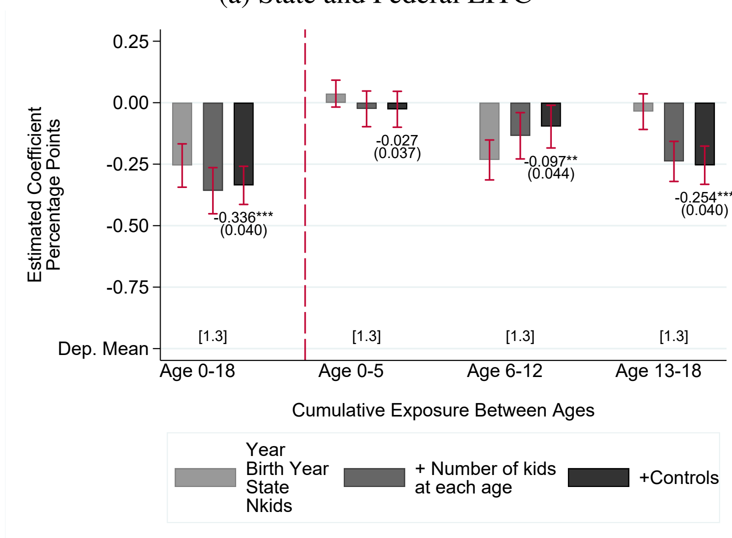
Notes: * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level and *** indicates statistical significance at the 1% level. The two-sample fuzzy RD estimate is calculated as the ratio of the reduced-form and first stage coefficients. Standard errors were calculated using the delta method under the assumption of zero covariance between the first-stage and reduced form estimates as in Dee and Evans (2003).

C Chapter 3 Appendix

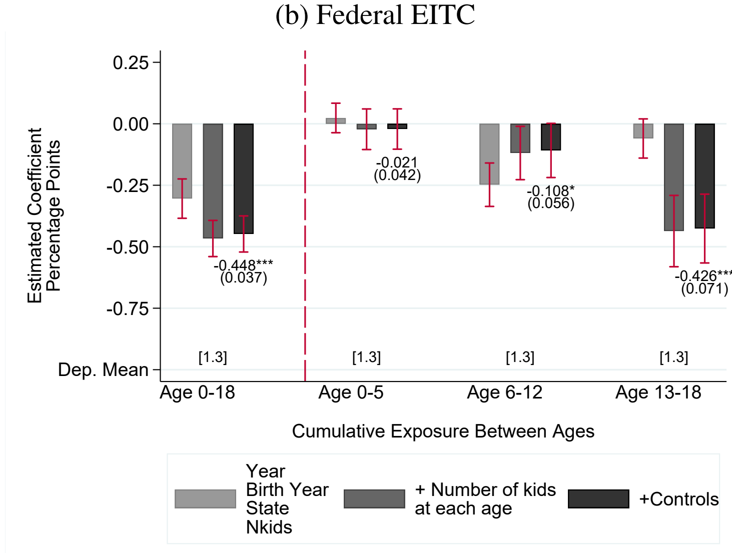
C.1 Figures

Figure C.1: Robustness of Estimates to Controls

(a) State and Federal EITC



(b) Federal EITC



Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Standard errors are clustered at the state level. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars.

C.2 Tables

Table C.1: Summary Statistics, CPS Sample

Variable	Mean	Std. Dev
SSI Recipient	0.008	0.089
Number of Kids in Family	2.083	0.795
Cumulative Federal EITC ($1000) Annualized	1.728	1.570
Cumulative Federal and State EITC ($1000) Annualized	1.820	1.696
Contemporary Federal EITC ($1000)	2.605	2.003
Contemporary Federal and State EITC ($1000)	2.766	2.194
Medicaid Eligible to Age 18	0.432	0.495
Adult Disability	0.017	0.009
Adult Unemployment	0.046	0.016
Black	0.145	0.352
Hispanic	0.122	0.327
White	0.801	0.399
Female	0.485	0.500
Low Educated Mother	0.183	0.386
Single Mother	0.276	0.447
N	463,351	

Data source: Current Population Survey 1975-2017.
Notes: Data include children born between 1952 and 2002 who were between the ages of 15 and 18 at the time of the survey and are classified as the child, stepchild, or grandchild of the head of household.

Table C.2: Estimates of Cumulative Maximum Federal EITC on Other Outcomes, Children Age 15-18

Dep. Variable	Cognitive Impairment		Physical Impairment		Visual Impairment		Mom Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cumulative EITC (0-18)	0.0001		0.0015**		-0.0007		-0.0031	
	(0.0010)		(0.0006)		(0.0006)		(0.0037)	
Cumulative EITC (0-5)		-0.0005		0.0012***		-0.0004		-0.0041**
		(0.0008)		(0.0004)		(0.0005)		(0.0019)
Cumulative EITC (6-12)		0.0008		0.0003		-0.0001		-0.0060
		(0.0013)		(0.0009)		(0.0009)		(0.0049)
Cumulative EITC (13-18)		0.0006		-0.0004		-0.0002		0.0078**
		(0.0012)		(0.0006)		(0.0006)		(0.0038)
Dep. Mean	0.044	0.044	0.01	0.01	0.013	0.013	0.656	0.656
N	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085	1,892,085
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.

Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. Each health outcome is an indicator for whether the child reported a cognitive, physical, or visual impairment that impedes work. Mom Employed is an indicator that the child's mother reports employment in the survey. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.3: Estimates of Cumulative Maximum Federal EITC on SSI Award, by Child Sex and Race

Dep. Variable	Girls		Boys		White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cumulative EITC (0-18)	-0.0038***		-0.0051***		-0.0047***		-0.0038**		-0.0029***	
	(0.0005)		(0.0007)		(0.0004)		(0.0019)		(0.0010)	
Cumulative EITC (0-5)		0.0004		-0.0008		-0.0002		0.0001		0.0003
		(0.0006)		(0.0006)		(0.0004)		(0.0017)		(0.0009)
Cumulative EITC (6-12)		-0.0019***		-0.0004		-0.0002		-0.0036*		-0.0009
		(0.0007)		(0.0008)		(0.0004)		(0.0019)		(0.0010)
Cumulative EITC (13-18)		-0.0033***		-0.0051***		-0.0058***		-0.0009		-0.0036***
		(0.0010)		(0.0010)		(0.0007)		(0.0029)		(0.0010)
Dep. Mean	0.0100	0.0100	0.0150	0.0150	0.0110	0.0110	0.0240	0.0240	0.0110	0.0110
N	911,042	911,042	981,043	981,043	1,400,419	1,400,419	220,741	220,741	315,281	315,281
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.

Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.4: Estimates of Cumulative Maximum State & Federal EITC on Other Outcomes, Children with Low Educated Mother

Dep. Variable	Cognitive Impairment		Physical Impairment		Visual Impairment		Mom Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cumulative EITC (0-18)	0.0014		0.0015*		0.0003		-0.0114***	
	(0.0014)		(0.0008)		(0.0006)		(0.0042)	
Cumulative EITC (0-5)		-0.0002		0.0012**		-0.0003		0.0017
		(0.0010)		(0.0005)		(0.0005)		(0.0032)
Cumulative EITC (6-12)		0.0033**		-0.0006		0.0005		-0.0099*
		(0.0016)		(0.0010)		(0.0010)		(0.0057)
Cumulative EITC (13-18)		-0.0015		0.0009		0.0002		-0.0065
		(0.0017)		(0.0008)		(0.0010)		(0.0054)
Dep. Mean	0.0470	0.0470	0.0120	0.0120	0.0160	0.0160	0.6430	0.6430
N	797,043	797,043	797,043	797,043	797,043	797,043	797,043	797,043
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.

Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. Each health outcome is an indicator for whether the child reported a cognitive, physical, or visual impairment that impedes work. Mom Employed is an indicator that the child's mother reports employment in the survey. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.5: Estimates of Cumulative Maximum Federal EITC on Other Outcomes, for Children with Low Educated Mother

Dep. Variable	Cognitive Impairment		Physical Impairment		Visual Impairment		Mom Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cumulative EITC (0-18)	0.0028*		0.0018*		-0.0000		-0.0163***	
	(0.0015)		(0.0011)		(0.0008)		(0.0050)	
Cumulative EITC (0-5)		-0.0009		0.0016**		-0.0005		0.0018
		(0.0011)		(0.0007)		(0.0007)		(0.0031)
Cumulative EITC (6-12)		0.0058***		-0.0002		0.0008		-0.0151**
		(0.0019)		(0.0015)		(0.0013)		(0.0061)
Cumulative EITC (13-18)		-0.0016		-0.0003		-0.0001		-0.0087*
		(0.0020)		(0.0009)		(0.0013)		(0.0047)
Dep. Mean	0.0470	0.0470	0.0120	0.0120	0.0160	0.0160	0.6430	0.6430
N	797,043	797,043	797,043	797,043	797,043	797,043	797,043	797,043
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. Each health outcome is an indicator for whether the child reported a cognitive, physical, or visual impairment that impedes work. Mom Employed is an indicator that the child's mother reports employment in the survey. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.6: Estimates of Cumulative Maximum State & Federal EITC on SSI Award, by Child Sex and Race Children with Low Educated Mother

Dep. Variable	Girls		Boys		White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cumulative EITC (0-18)	-0.0019		-0.0027**		-0.0025**		-0.0016		-0.0033**	
	(0.0011)		(0.0012)		(0.0010)		(0.0020)		(0.0014)	
Cumulative EITC (0-5)		0.0014**		-0.0007		-0.0001		0.0034**		0.0000
		(0.0007)		(0.0009)		(0.0005)		(0.0015)		(0.0013)
Cumulative EITC (6-12)		-0.0014		0.0011		0.0009		-0.0028		0.0003
		(0.0012)		(0.0011)		(0.0010)		(0.0022)		(0.0010)
Cumulative EITC (13-18)		-0.0028***		-0.0039**		-0.0042***		-0.0037		-0.0048***
		(0.0011)		(0.0016)		(0.0013)		(0.0033)		(0.0012)
Dep. Mean	0.0120	0.0120	0.0190	0.0190	0.0130	0.0130	0.0300	0.0300	0.0100	0.0100
N	386,842	386,842	410,201	410,201	560,511	560,511	93,380	93,380	199,895	199,895
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.

Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.7: Estimates of Cumulative Maximum Federal EITC on SSI Award, by Child Sex and Race Children for Children with a Low Educated Mother

Dep. Variable	Girls		Boys		White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cumulative EITC (0-18)	-0.0026**		-0.0040***		-0.0036***		-0.0012		-0.0030**	
	(0.0011)		(0.0014)		(0.0009)		(0.0024)		(0.0015)	
Cumulative EITC (0-5)		0.0019*		-0.0008		0.0001		0.0035*		-0.0000
		(0.0010)		(0.0010)		(0.0006)		(0.0021)		(0.0014)
Cumulative EITC (6-12)		-0.0009		0.0021		0.0022**		-0.0037		0.0014
		(0.0012)		(0.0015)		(0.0009)		(0.0031)		(0.0015)
Cumulative EITC (13-18)		-0.0056***		-0.0071***		-0.0081***		-0.0033		-0.0062***
		(0.0019)		(0.0019)		(0.0015)		(0.0055)		(0.0012)
Dep. Mean	0.0120	0.0120	0.0190	0.0190	0.0130	0.0130	0.0300	0.0300	0.0100	0.0100
N	386,842	386,842	410,201	410,201	560,511	560,511	93,380	93,380	199,895	199,895
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.

Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level and regressions are weighted using the survey person weight. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.8: Estimates of Cumulative Maximum Federal & State EITC on SSI Award, Unweighted

	Federal EITC		State & Federal EITC	
Dependent Variable	(1)	(2)	(3)	(4)
Cumulative EITC (0-18)	-0.0056***		-0.0042***	
	(0.0004)		(0.0004)	
Cumulative EITC (0-5)		-0.0004		-0.0007**
		(0.0003)		(0.0003)
Cumulative EITC (6-12)		-0.0016***		-0.0009*
		(0.0005)		(0.0005)
Cumulative EITC (13-18)		-0.0044***		-0.0028***
		(0.0006)		(0.0005)
Dep. Mean	0.0130	0.0130	0.0130	0.0130
N	1,892,085	1,892,085	1,892,085	1,892,085
State FE	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Age x Nkids FE	YES	YES	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equation (3.1) or (3.2). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Cumulative EITC is annualized across all years within the stated age range (e.g. 0-5, 6-12, 13-18, 0-18). EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.

Table C.9: Difference-in-Differences and Maximum Credit Estimates of EITC on SSI Awards, Unweighted

Dependent Variable	ARRA 2009 Diff-in-Diff (1)	Contemporary Federal EITC (2)	Contemporary State & Federal EITC (3)
Treatment Coef.	-0.0008*	-0.0041***	-0.0029***
	(0.0004)	(0.0004)	(0.0003)
Dep. Mean	0.0130	0.0150	0.0150
N	1,503,112	2,114,608	2,114,608
State FE	YES	YES	YES
Birth Year FE	YES	YES	YES
Year FE	YES	YES	YES
Nkids FE	NO	YES	YES

Date source: American Community Survey years 2001-2017.
Notes: Each column is a separate regression of equations (3.3) and (3.4). Heteroskedasticity robust standard errors are clustered at the state level and reported in parentheses. Observations are at the individual level. The specification includes the following controls: child gender, child race, the fraction of the adult population unemployed, the fraction of the adult population on SSI, whether the child has a single mother, and whether the child has a low educated mother. Contemporary EITC is measured in thousands of 2013 real dollars. SSI is an indicator for the child reporting positive income from the Supplemental Security Income program. * indicates statistical significance at the 10% level, ** indicates statistical significance at the 5% level, and *** indicates statistical significance at the 1% level.